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SOME PHILOLOGICAL ASPECTS OF ANTHROPOLOGICAL RESEARCH.[1]

IT is, perhaps, partly due to accident that American anthropologists meet to-day, for the first time, jointly with the American Philological Association and with the Archeological Institute of America. Nevertheless, I welcome our joint meeting as a significant fact, because it emphasizes the growing feeling of anthropologists that our science may profit from the methods developed by classical and oriental archeology, and by the well-established methods of philological and linguistic research. We hope that it may also express the growing feeling among philologists and archeologists of the importance of anthropological research for their own studies.

Our cooperation with your societies indicates a radical change in the attitude of students of anthropology. Up to the present time we have affiliated with societies representing the natural sciences and psychology. This is due to the development of modern anthropology under the stimulus of the theory of evolution, and to the important incentives that it has taken from the methods pursued by the natural sciences. It has been the endeavor of anthropologists to discover universal laws, like the laws of physics and of chemistry. This tendency has been somewhat modified by the influence of those historical methods in the biological sciences which endeavor to

[1] Paper read at the joint meeting of the Anthropological Association, the Archeological Institute, and the Philological Association, at Ithaca, N. Y., December 28, 1905.

explain the present types as the result of a long-continued development from previous forms.

Owing to the peculiar conditions under which it has grown up, American anthropology has been devoted almost exclusively to the study of North American problems. As we have penetrated more deeply into these problems we have observed that the general laws for which we have been searching prove elusive, that the forms of primitive culture are infinitely more complex than had been supposed, that a clear understanding of the individual problem can not be reached without taking into consideration its historical and geographical relations.

As this new point of view becomes more and more clearly established, the tendency must increasingly develop of turning away from the comparative methods of the natural sciences, and taking up more and more systematically the methods of history. While the first problem that presented itself to the anthropologist was the puzzling sameness of traits of culture in remote parts of the world, and while his endeavor was directed towards the discovery of the psychological causes that bring about such sameness, we begin to be inclined to view each cultural trait not primarily in comparison with parallel traits found in remote regions, but rather in connection with the direction taken by the whole culture of a tribe or a people. We begin to see that sameness of cultural traits does not always prove genetic relation, but that diverse traits have often tended to converge, so as to develop similar thoughts and activities; while, on the other hand, other traits have tended to diverge, and to assume in different regions different forms.

With the appreciation of this fact, the necessity of a much more thorough and detailed knowledge of primitive culture is recognized. While hitherto we have been satisfied with disconnected fragments of observations on the customs of the various tribes, we begin to see more and more clearly that the student must have a full grasp of all the forms of culture of the people he studies, before he can safely generalize.

It would seem to me that the classical archeologist or the classical philologist must always have an indulgent smile when he hears of serious anthropological studies carried on by investigators, who have neither the time, the inclination, nor the training to familiarize themselves with the language of the people whom they study. According to the canons of philological research, would not the investigator who is not able to read the classics be barred from the number of serious students? Would not the historian who investigates the history of the civilization of the middle ages, and who can not read the literature of that period, be excluded from the number of investigators? Would not the student of Oriental countries, who has to rely for his information on the assistance of interpreters, be considered an unsafe guide in the study of these countries? Still, this is the position which has confronted anthropology up to the present time. There are very few students who have taken the time and who have considered it necessary to familiarize themselves sufficiently with native languages to understand directly what the people whom they study speak about, what they think and what they do. There are fewer still who have deemed it worth while to record the customs and beliefs and the traditions of the people in their own words, thus giving us the objective material which will stand the scrutiny of painstaking investigation. I think it is obvious that in this respect anthropologists have everything to learn from you; that until we acquire the habit of demanding such authenticity of

our reports as can be guaranteed only by philological accuracy of the record, can we hope to accumulate material that will be a safe guide to future studies.

The time must come when we must demand that collections of traditions obtained by means of the garbled English of interpreters, descriptions of customs not supported by native evidence, records of industries based only on the objective observation of the student, must be considered inadequate, and that we must demand from the serious student the same degree of philological accuracy which has become the standard in your sciences.

It is true that in many cases this ideal can not be obtained. The general breakdown of native culture, the fewness of numbers of certain tribes, the necessity of rapidly accumulating vanishing material, may sometimes compel the student, much against his will, to adopt methods of collecting which he recognizes as inadequate. Nevertheless, an important step forward will be made if we acknowledge that such collections are makeshifts that should be supplemented as soon as feasible, and wherever feasible, by more painstaking records.

Taking this standard as a guide, we must acknowledge that very little, if any, of our literature is sufficiently authentic. Perhaps the most valuable material that has been collected from this point of view is the long series of texts obtained from the Ponka and Omaha by the late James Owen Dorsey. It is true that they embrace only a limited aspect of the life of the tribe, but so far as they go, they give us a deep insight into the mode of thought of the Indian. In the whole range of American anthropological literature there is hardly anything that may be compared to this publication. We have short series of texts from a few tribes which are highly welcome, but as they stand, they are but fragments of what is required. The tribes thus treated are the Sioux, the Klamath of Oregon, the Kwakiutl, the Chinook and the Haida, and there is also a considerable amount of material available from the Eskimo, although most of the published material in that language is overlaid with Danish culture.

If we consider the whole range of native life that should be treated in the same manner, we see how utterly inadequate the available collections are. To take, as an instance, the best—that of Mr. Dorsey—the contents of the volume are a collection of myths, records of war-expeditions and a long series of personal letters. These topics cover only a narrow range of the life of the Ponka. The whole material culture, their knowledge of the country and of neighboring tribes, their rituals and ritualistic myths, their social organization, their beliefs have not been recorded, and are known to us only by brief notes collected by the author.

If we acknowledge the correctness of the requirements here outlined, the work that is before us is stupendous. Let me remind you that in North America we have probably about fifty-five distinct linguistic stocks and at least three hundred and fifty distinct dialects. If full information on all of these is to be gathered, the most intensive work of a great number of students is immediately required, because the information is rapidly disappearing, and probably almost all of it will be lost inside of fifty years. The demand for thoroughness of method of collection must, therefore, be brought forward with great emphasis.

I have spoken here of the linguistic and historical method only as an adjunct of ethnological research. It is, however, true that the linguistic problem itself is one of intense interest, and one which will gain by a knowledge of the methods applied by

Indo-European philology. The forms of the Indian languages differ enormously. It is often assumed that there is one type of American language, but even a superficial knowledge of representative dialects of American stocks shows that much greater than their similarities are their differences, and that the psychological basis of morphology is not by any means the same in the fifty-five stocks that occur on our continent. The scientific problems which are involved in their study have hardly been touched. I must say with regret that the anthropologist of the present day is not the man to solve these problems; that we require not only the stimulating example of philologists, but also their assistance. *You* must give preliminary training to the men who are to take up the problems of American languages; because the centuries of experience and of labor that have been bestowed upon the development of philological methods have given you the advantage of settled lines of approach of linguistic problems. If you are willing to lend us your assistance in this important investigation, I foresee a field of important discoveries which will in their turn be of great benefit to the science of language. The psychological foundation and morphological development of American languages are so peculiar that their study must be a revelation to the student of Indo-European or of Semitic languages. Well-known problems which you have discussed for years appear in new aspects, and broad points of view for discussion of linguistic questions present themselves readily to the student who takes up the types of language peculiar to our continent.

I beg to be allowed to make the direct appeal to you here, asking you to turn the attention of your younger students to this promising field. It is virgin soil, and he who takes up the subject with a fairly adequate equipment is sure to find most ample compensation for his toil in new and valuable discoveries. Without your help we shall never be able to solve this task, which requires the speediest attention and the cooperation of many investigators.

When we once have the equipment such as I have tried to outline, when we have investigators who collect the material in authentic form, and when we have students who will apply themselves to a painstaking analysis of the collected data, our problems will probably appear in entirely new light. The connection between prehistoric archeology and modern ethnology will necessarily become of the same character as the relation between early classical archeology and the study of classical literature. It is true our problems will always remain more obscure and more difficult than yours, because we have no historical documents that carry us back through any considerable length of time, while, by the necessities of the case, we are compelled to use, instead of historical methods, geographical methods. We have to trace historical transmission and historical contact by studies of geographical distribution. Often we find ourselves confronted by contradictory evidence, but, notwithstanding all these difficulties, the little progress that has been made during the last twenty years indicates plainly that, from this point of view, the historical problem of anthropology may be approached with the hope of a certain amount of success and that we may be able to reconstruct important historical facts.

I have given expression here to the growing need of the introduction of sounder philological methods of collection and of historical methods in the treatment of anthropological problems. I do not wish to be understood as advocating a dissociation of anthropology from psychology and the natural sciences. The source from which modern anthropology has grown up, the problems that have presented themselves to

us from the point of view of the student of natural sciences, who takes human nature for his subject, are novel and are important. They touch upon the fundamental questions underlying the history of human civilization, and their clear formulation must be recognized as a distinct contribution of anthropology to the scientific development of the day. Most important among these results is, perhaps, the recognition of the fundamental sameness of the traits in human culture the world over and of the psychic unity of mankind. The data on which these conclusions are based have not been without influence upon modern history and modern philology, and I do believe that if we have to learn much from you, we can also offer in return a point of view that will prove fertile in your work. The modification of the theories of the development of mythology, the better appreciation of the earliest development of Greek and Oriental culture, would hardly have come about if anthropological points of view had not made themselves felt in the minds of archeologists and philologists. If it must be *our* endeavor to broaden our methods by learning from *you* the foundations of historical research, we may offer to you also the results of many honest attempts of applying the methods of natural science to the phenomena of human culture.

Let us hope that our first joint meeting may introduce a period of closer contact, of greater readiness on the part of anthropology to learn from her older sisters, and of a better understanding of the aims of anthropology by students of language and of history.

Franz Boas.

THE CONDITIONS OF ADMISSION TO COLLEGE.[1]

The topic assigned me springs out of a paper given at the July meeting of the National Educational Association upon another topic assigned me: 'Which is better, the western plan of admitting students to colleges and universities by certificates from duly inspected secondary schools, or by the eastern method of admitting only by examinations conducted by representative boards or otherwise?' An abstract of my treatment of this subject may best serve as an introduction to the topic of to-day:

Within a few years it may be determined which plan, with all it implies in shaping far-reaching educational ideals and practises, shall be national. The term 'western' and 'eastern' must not import provincial pride, or sound a note of sectionalism.

The examination by the separate college of the individual candidate, giving 'personal contact,' has failed on account of the increase in numbers.

The college entrance examination board organized in 1900, examined some 2,100 candidates this year—a Lilliputian effort as compared with the need to examine some 66,000 candidates. It has all the disadvantages of massed examinations, making it a gamble for the entering student and of judgment simply upon paper.

The New England college certificate board cares for some 2,000 candidates and has the virtue of resting upon the judgment of the teacher acquainted with the pupil. But it lacks any note of nationality and is without provision for any proper inspection and accrediting of the schools.

President Hadley has just announced that for the present Yale will adhere to the separate examination system. Yet President Hadley personally would give teachers of proved ability the opportunity to recommend for provisional admission to the freshman class. Thus President Hadley is not far from the kingdom of the outright accrediting system for which we hope he may become a leader, not only amongst his brethren of the eleven colleges in the New England college entrance certificate board, but throughout the nation. The whole thing might be done if

[1] Can there be a coordination of the examining, certificate, and accrediting (including school inspection) systems for admission to college looking toward a common or national administration in the interests of students, colleges and the preservation of the standards? Discussion opened by President George E. MacLean, of the State University of Iowa, at the meeting of the National Association of State Universities, Washington, D. C., November 13–14, 1905.

Commissioner Draper and President Butler became his coadjutors.

The so-called 'western' is really a development from the German plan. It, in some form, logically accompanied a state public school system crowned by a state university. It has been adopted also by private universities so that it covers the entire territory from the Ohio to the Pacific, and overflows into southern and eastern states. At present there are twelve state or state university inspectors in as many great western states—supplemented by visitors from great private institutions. In the north central association of colleges and secondary schools, there has been for six years a commission on secondary schools and college entrance requirements, at the heart of which is a board of high school inspectors. Uniform standards and entrance blanks have been prepared. But now a list of first-class schools meeting the standards of the commission is becoming an accredited list throughout the entire northwest.

The accrediting system has raised the standard of the work done. It has linked the secondary school into one system with the college. It has given an increase of students entering college, and with better average preparation. At the university of Pennsylvania in the fall of 1901, of those entering by examinations 49 per cent. were conditioned as against only 29 per cent. of certified students. An investigation by Principal Ramsay showed that the certificated students excelled in mental ability five to one. In the general performance of college duties they excelled three to one. Professor Whitney, of Michigan, found that the average standing of the certified student was more than 1.5 per cent. higher than for the examined student.

Professor T. Gregory Foster in the report of the Alfred Mosley commission rejoices that it is a fundamental principle in American universities that the man who is fit to teach is also to be trusted to examine his own students. He says the accrediting system of the middle west is a most significant plan and one rapidly spreading into the east.

In the states where it has been adopted the whole educational system has been unified and strengthened. The barriers between various grades of teachers are being removed. The teaching of all classes of teachers is thereby made more direct, more stimulating and attractive to students. The accrediting system as versus the older leaves the teacher and the taught free and thereby stimulates to better training.

Professor Foster quotes President Harper as opposed to the accrediting system when he left Yale, but now as a firm believer in it as a result of his experience. The professor concludes, 'It is perhaps one of the most noteworthy contributions of America to educational progress.'

What we do we must do quickly. A national system (meaning thereby governmental coordination and possible inspection in harmony with the voluntary cooperation in many western states, concatenating secondary schools, colleges and universities) will give modern interstate educational privileges, long needed to keep up with interstate commerce and life, and heightening national ideals and power.

That there can be local coordination of the examination, certificate and accredited systems for admission to college is clear, because it is already accomplished in fact. It is true in many institutions. We have an excellent illustration in the report for 1904–5 of President Schurman of Cornell. He says:

In the year 1904–05 the number of matriculants presenting certificates in satisfaction of the entrance requirements was 317, and the number of schools they represented was 154. It is sometimes alleged that the scholarship of students admitted on certificate is lower than that of students who are required to pass examinations. But the experience of Cornell University does not support this contention. And consequently the faculty sees no reason for disturbing an arrangement which, as Dean Crane points out, 'is convenient both for the schools and the university.' Nevertheless, Cornell has from the first cooperated with the College Entrance Examination Board and many of its matriculants enter by the way of the board's examinations. Thus of 1,817 taking the board's examinations in 1904 not less than 251 announced their intention to enter Cornell University. A third avenue to the university is the regents' diploma for New York state students; and with this credential 238 matriculated in 1904–05. There remains the method of entrance by examinations at the university, which are now given only in September, and of this method 27 availed themselves in 1904–05. The remaining members of the freshman class were admitted on credentials from other universities and colleges, or on medical students' certificates.[2]

[2] Cornell University, President's Report, 1904–05, pp. 36–37.

The Cornell case, showing that there can be a local coordination, shifts the emphasis of our discussion to the question whether there can be a common or national administration in the interest of students, colleges and the preservation of standards. That there are a tendency and need and a longing for a common, and indeed a national administration, is evident. The tendency springs from axioms of economic science like that of 'planless production makes waste.' The spirit of this era of cooperation and combination intensifies the longing and the need becomes positive as rapidity of transportation and communication facilitates migration. The unifying of the republic, the emphasizing of American ideals with a deepening consciousness of our world-wide relations, unite the tendency, longing and need into an aspiration and positive demand for the recognition and development of a national system of education.

This appears in unexpected ways. President Schurman in his report, referring to Mr. Carnegie's professorial pensions and Mr. Rockefeller's subsidies for general education in colleges, says:

> Both philanthropists have risen above the idea of a single institution and have grasped the conception of a national system of higher education. And the bounty is as splendid as it is unparalleled in the history of higher education in America. But relatively to the ideal of an efficiently organized system of higher education in the United States, it is only a beginning.[3]

President Hadley's last report,[4] true to the spirit of Yale, breathes with the thought of becoming national. He would gladly appropriate the genius of the state university. He cites Yale's work in forestry as 'including the kind of public work which makes the modern university something more than a mere group of schools and elevates it to its highest possible rank —that of a public servant.' He dwells upon considerations of public duty as affecting the requirements for admission. He says:

> If the Yale requirements should get so far out of the line of work furnished by the better kind of high schools in the country that we could not expect to get boys from those schools, we should soon become a local institution. Yale would be a school for boys of one kind of antecedents, instead of for boys of all kinds of antecedents; and as soon as it became a school for boys of one kind of antecedents only, it would lose its value as a broadening influence to its students and as a factor in the life of the whole nation.
>
> Our policy with regard to entrance requirements is thus governed by two separate considerations: our duty to ourselves of not admitting boys except those who are able to do the kind of work which will be required of them, and our duty to the public of admitting all kinds of boys who can do this, on as equal terms as possible. Our student body must be at once hard working and national.[5]

He then makes this surprising application of this splendid doctrine:

> In order to make ourselves national we admit boys to our undergraduate courses by examination only and not by certificate. We believe that the examination method is fairer to boys who come from distant places. The certificate system is the natural one for the state university, which draws its pupils chiefly from the schools of one locality and can inspect and examine those schools; but if a national university tries to apply this system it gives either an unfair preference to the boys from schools near at hand, or an inadequate test to the boys from remote ones.[6]

The plausibility of this conclusion disguises the logic of the actual present conditions. As if one institution could become national by refusing recognition to the arrangements of great national groups of secondary schools and colleges like those of the New England College Entrance Certificate Board, and those of the College

[3] Cornell University, President's Report, 1904–05, p. 74.

[4] SCIENCE, October 27, 1905, p. 514 and following.

[5] SCIENCE, October 27, 1905, p. 518.

[6] *Ibid.*, p. 519.

Entrance Examination Board of the Association of Colleges and Preparatory Schools in the Middle States and Maryland, and the accrediting system of the state and private universities and colleges, particularly as unified through the Commission of Secondary Schools and Entrance Requirements, with its board of high school inspectors, in the North Central Association of Colleges and Secondary Schools! How can an institution hope to become national by becoming isolated and local in setting its own examinations? Under this idea confusion becomes worse confounded as institutions multiply with aspirations to be national, but insisting upon making their own examinations. What a reversion this is is evident in the light of the approximation to something national which began to appear through the three or four great provincial organizations just mentioned, covering most of the national territory. By the same token that the certificate system is a natural one for the state university, it would seem to be the one for a national university.

The great state universities draw their students from many states and countries and have learned by a system of comity that they can safely accept the inspection and accrediting of sister state universities. In fact, with the exception of but three prominent institutions, Harvard, Yale and Princeton, have we not arrived at a practical coordination of the examining, certificate and accrediting system in that the institutions in the great provincial organizations above referred to, upon occasion accept the testimonials issued by the authorities of any one of these systems? It only remains to see that what the student migrating from one of these great provincial groups to another accomplishes in entering an institution, is safeguarded from fraud or misinterpretation, and that positively uniform and high standards are maintained by the establishment of a proper channel for exchange of documents.

A common administration could be established through a delegacy consisting of secretaries of the existing provincial organizations. Indeed, the College Entrance Examination Board affords a hint as to a way to do it. It provides that representatives of the secondary schools on that board may be appointed by the New England Association of Colleges and Preparatory Schools, the Association of Colleges and Preparatory Schools of the Middle States and Maryland, the Association of Colleges and Preparatory Schools of the Southern States and the North Central Association of Colleges and Secondary Schools.

'Each association may appoint one secondary school representative for every three colleges and universities represented in such association and admitted to membership in the board';[7] but under the limitation that the colleges must be admitted by vote of the board to membership, and that the number of secondary schoolmen appointed by any one association shall in no case exceed five.

The scheme of the College Entrance Examination Board strictly interpreted, it will be seen, is not automatic; it requires election and is exclusive, and necessarily under their scheme, of anything but the examination system.

Let these associations inaugurate a movement by having a conference of representatives from the associations, to which also representatives of Harvard, Yale and Princeton might be asked.

The first step of a common administration, coordinating the examining, certificate and accrediting systems, seems relatively easy. When we import the term *national* administration in the higher or govern-

[7] *Educational Review*, October, 1904, p. 265.

mental sense, the difficulties are greatly increased and differences of opinion will multiply. For one, however, I venture to believe that the movements under way will not rest until in some conservative way we have a national attachment—that is, a governmental point of attachment. It must be conceded in the words of Commissioner Andrew S. Draper, upon the legal status of public schools, 'that while they are not national, neither are they local institutions —rather are they state institutions.'[8] In another place he says: at the close of the Revolution, 'it was easily conceived to be a function of government to *encourage* schools.'[9]

> Since the American school system has come to be supported wholly by taxation, it has come to depend upon the exercise of a sovereign power. In the United States the sovereign powers are not all lodged in one place. Such as have not been ceded to the general government are retained by the states. The provision and supervision of schools is one of these. Hence the school system, while marked by many characteristics which are common throughout the country, has a legal organization peculiar to each state.[10]

Great as are the systems of state schools covering the most of the land and culminating in New York in the most complete state system, unifying the public and private institutions, they do not satisfy, but on the contrary they feed the hunger for a national system, but better, for a federal coordination of the state systems. The state of New York blazes the way for an analogous plan blending the private and state institutions and relating them to the federal government.

An objector will recall the legal status above conceded, and specifically that the Bureau of Education is only advisory, a collector of statistics and an educational clearing house.[11] But as 'necessity is the mother of invention,' and brought forth after the Civil War with the need of education in the south for the freedman and for the immigrants, through the advocacy of a Barnard and a Garfield, in 1867 the Bureau of Education, so again, following the Spanish-American War, necessity for education in our new possessions, including Alaska, has tended to a development of the Bureau of Education.[12]

The committee on resolutions of the National Educational Association, Nicholas Murray Butler, chairman, brought in a report adopted by the association earnestly urging

> Upon the Congress the wisdom and advisability of reorganizing the Bureau of Education upon broader lines; of erecting it into an independent department on a plane with the Department of Labor; of providing a proper compensation for the Commissioner of Education; and of so constituting the Department of Education that, while its invaluable function of collating and diffusing information be in no wise impaired, it may be equipped to exercise effective oversight of the educational systems of Alaska and of the several islands now dependent upon us, as well as to make some provision for the education of the children of the tens of thousands of white people domiciled in the Indian Territory, who are without any educational opportunities whatever.
>
> Such reorganization of the Bureau of Education and such extension of its functions we believe to be demanded by the highest interests of the people of the United States, and we respectfully but earnestly ask the congress to make provision for such reorganization and extension at its next session. The action so strongly recommended will in no respect contravene the principle that it is one of the recognized functions of the national government to encourage and to aid, but not to control, the educational instrumentalities of the country.[13]

Dr. Butler in an editorial in the *Educa-*

[8] *Proceedings and Transactions*, N. E. A., 1889, p. 183.

[9] 'Education in the United States,' edited by Nicholas Murray Butler, Vol. I., 1900, p. 5.

[10] *Ibid.*, pp. 17–18.

[11] 'History of Education in the United States,' Dexter, p. 202.

[12] 'Addresses and Proceedings,' N. E. A., 1901, p. 435.

[13] 'Addresses and Proceedings,' N. E. A., 1900, p. 31.

tional Review, 1901, follows up the subject conservatively:

> Questions of erecting the Bureau of Education into an executive department, with a seat in the Cabinet, as was proposed by Senator Hansbrough's bill, introduced into the Fifty-sixth Congress, or of organizing it on the same plane as the Department of Labor, are not necessarily involved, and may wisely be postponed until public opinion on the subject is better informed and more clearly formulated. All immediate necessities could be met by an amendment of existing law that should provide for a bureau of education with two divisions: a division of statistics and reports, to do the work now done by the bureau; and a division of supervision and administration, to take up the oversight of the school systems of Alaska, of the white residents in Indian Territory, of Porto Rico and of the Philippine Islands.[14]

With our eyes opened by foreign needs in this era of a new nationalism, would it not be well to turn them upon our greater domestic educational needs and the needs of our own white children for developing the bureau as shown by the subject we have in hand. Some sense of such needs stirred this association a year ago to appoint a committee consisting of Presidents Van Hise and Jesse to draft a memorial to enlarge the function of the Bureau of Education.[15] Without an amendment to the act establishing the Bureau of Education, might it not find authority with comparatively small addition to its expenditures, to act in place of, or in conjunction with, the delegacy above proposed? The law says it shall 'aid the people of the United States in the establishment and maintenance of efficient school systems and otherwise promote the cause of education throughout the country.' Let it federate and coordinate our present school systems. Let it endorse and promulgate national standards. Local systems and institutions would be free to accept them or not; indeed, national inspectors might complement state and institutional inspectors; the national inspectors visiting upon invitation and without authority, as indeed is the case with the majority of state inspectors. The national inspectors could validate the work of local inspectors for remote parts of the country. The individual colleges would upon occasion, now in this, now in that subject, be at liberty, as they now are even in the most highly developed accrediting systems, to give examinations to an entering student.

In fine, the proposals of this paper apply the doctrine of evolution. We grow from the systems we now have. We correlate them. We leave liberty to each institution and group of institutions to favor the system or lack of system it may have. All that is asked is an open-door policy instead of an exclusive one. Ultimately the best system or combination of systems will survive. In the meantime, there will be a germinal genuine American system looking toward a national one in harmony with our new nationalism.

George E. MacLean.
The State University of Iowa.

[14] *Educational Review,* Vol. 21, 1901, p. 528.

[15] *Transactions and Proceedings,* National Association of State Universities of America, 1904, p. 23.

EFFECTIVE PROTECTION FOR THE LOBSTER FISHERY.

The main biological facts concerning the lobster are now well in hand, and form a logical basis for the protection of the fisheries of this animal.

In restricting the size of marketable lobsters the following methods are entitled to consideration by the legislator who regards the question upon its scientific merits alone: (1) partial protection of young and adult, with emphasis upon the young; (2) partial protection of adult and young, with emphasis upon the adult. Such regulations may be supplemented by various other prohibitions, relating to close seasons, the destruction of 'berried' females and the sale

of broken lobster meat. We are now mainly concerned with the restrictions placed upon the fisherman in dealing with lobsters which enter his traps. The protection of the young alone, or what is the same thing, unrestricted slaughter of adults, would be equivalent to slaying the goose which lays the golden eggs and must be ruled out at once, for to get young we must have eggs, or, what is the same thing, adults which produce the eggs, and the more of them the better.

Protection of the adult alone is neither practicable nor desirable, for the markets should be supplied with animals of fair size, and the period of sexual maturity fluctuates between rather wide limits. It would, moreover, be folly to permit the unlimited sacrifice of the young of all sizes which could be enticed into the traps, although the fishery might be better able to stand such a drain than the wholesale sacrifice of adults.

The keepers of domestic animals practise what may be described as 'a judicious protection of the adult.' That is, the relative proportion of young to adults being known, a balance can be struck and maintained, or any desired ratio between them established. In marine animals like the lobster, this ratio between young and adult is an unknown and unknowable quantity, and this is why comparisons drawn between domestic animals, which are under human control, and the invisible inhabitants of the depths of the sea are likely to be misleading. No selection or balancing of numbers is possible in the way that the poultryman or ranchman maintains the integrity of his flocks or herds. The lobster is seldom seen except when caught in a trap and brought to the surface. The fishermen follow the lobsters in their movements to and from the shores, and when the animals which enter their traps become smaller and fewer, or cease altogether, they begin to wake up to the highly probable fact that the wild 'flock' has been exterminated. Yet the fisherman is not to blame for this, since the laws have sanctioned what practically amounts to an indiscriminate slaughter of the adult.

Thus we are left to choose between the methods given above. In the first, where the fullest protection is given to the young, the aim has apparently been to allow the adult to breed at least once before it is sacrificed. But this desirable end is frequently not attained because, as will be seen later, many animals pass the legal limits—nine to ten and one half inches—before becoming sexually mature. This method has been given more than a fair trial, and has proved sadly lacking. The second method, as stated above, essentially means protecting the adults permanently beyond a certain size, and the young up to a certain limit. Between these two permanently protected classes stand the immature or adolescent and the smaller adult animals, which alone it would be permissible to destroy. This plan was first proposed in 1901 by Dr. George W. Field.[1] He advocated a reversal of the existing policy of protecting chiefly the young, by placing the weight of restrictive laws upon the adult animal above a certain size, when it is becoming most prolific, and, therefore, most valuable to the fishery. This may be described as partial protection of the adult and young, with emphasis on the adult, and it must be admitted that such a method has all the weight of biological fact and sound common sense on its side. In the abstract of his report which was published in this journal,[2] the various remedies which have been tried in vain to instil new life into the waning fisheries are ably

[1] Report to the Massachusetts Commissioners of Fisheries and Game, 1902.

[2] SCIENCE, N. S., Vol. XV., pp. 612–616, April 18, 1902.

discussed. In this connection it is profitable to read also the discriminating remarks of the late Capt. Joseph W. Collins, in 1903.[3]

The existing laws for the regulation of the lobster fisheries (see method 1 above) are designed, as we have seen, to shield mainly the young, since they give but partial protection to the adult animal, it being illegal to possess, sell or destroy any lobster under nine or ten and one half inches in length, or any female beyond these limits, with external eggs. The larger limit of ten and one half inches is in favor in most of the states. Dr. Field would protect the young up to a certain length, as nine inches,[4] permit the capture of all adolescents and adults between nine and ten and one half inches, and permanently protect all adults beyond this size. That is, he would reduce the protection afforded the young, but greatly enhance that given to adults.

I formerly advocated the retention of the ten and one half inch law, and opposed any reduction of this standard, because under the present methods (see No. 1 above) this would cut out nearly every vestige of protection afforded adult animals, which, as was pointed out, is very little at best. On the other hand, I am heartily in favor of reducing the legal size-limit of marketable lobsters to nine inches, provided the larger adults are placed in a permanently protected class.

In dealing with the zoological side of the question the facts which chiefly concern us are: (*a*) the period of maturity of adult lobsters; (*b*) the number of eggs borne by the females, or the size of the broods, and (*c*) the frequency of spawning, or succession of broods.

[3] 'Report upon a Convention held at Boston, 1903, to secure Better Protection of the Lobster,' Boston, 1904.

[4] This in my opinion is much better than his earlier suggestion of six to ten inches.

I have found that the period of maturity is very variable as regards both age and size, female lobsters coming into the bearing condition between the size limits of approximately seven to twelve inches in length. Comparatively few animals lay eggs before reaching a length of nine inches, when their broods are still relatively small, while on the other hand the reproductive period is seldom deferred to the eleven- or twelve-inch stage. When ten to ten and one half inches long the female lobster has, as a rule, reached her first reproductive period, and many have carried two or three broods. We thus see why, according to present methods, by simply reducing the legal length-limit, we rob the adult of the very meager protection which it now enjoys.

The number of eggs produced increases with surprising rapidity from the very beginning of sexual maturity, the first batch of eggs being relatively small, whatever the size of the lobster. The average number of eggs produced by lobsters eight inches long is approximately 5,000, at ten inches 10,000, at twelve inches 20,000, and at fourteen inches nearly 40,000. Out of 532 animals examined at the ten-and-one-half-inch stage the smallest, largest and average number of eggs borne were 5,000, 36,000 and nearly 13,000. Lobsters fifteen and sixteen inches long have been taken off Cuttyhunk Island in Buzzard's Bay, for the use of the hatchery at Woods Hole, bearing nearly 100,000 eggs, all of which shows how rapidly the value of the lobster as a breeder increases after the nine- or ten-inch length is attained.

The male lobster matures as early as the female, and possibly somewhat earlier. It is certain that the female lobster may be impregnated at any time, and by more than one male; the sperm, moreover, possesses great vitality. As a rule the female lobster lays her eggs every other year, that is, the

reproductive cycle is not a one-year but a two-year period.

With respect to reproductive ability, and of the females in particular, we may divide the lobsters in the ocean into three classes, as follows: (1) young and adolescents mainly, from swimming larva to the nine-inch stage; (2) intermediate class—adolescents and adults—nine to ten and one half inches long; (3) adult class mainly, from ten and one half inches and upward in length.

The existing laws from New York to Maine vary but little, the legal length-limit being placed at nine or ten and one half inches; in some cases females in spawn are also protected, and there is a close season in Rhode Island.

In the Dominion of Canada and the maritime provinces the legal size varies from eight to ten and one half inches, while in the former territory seven distinct close seasons of varying limits are maintained in certain geographical districts, extending from late spring to midsummer on the one hand, and from winter to spring on the other (beginning May 30 to August 10 and ending December 15 to May 25). Notwithstanding these and various supplemental regulations, the fishery in the dominion has steadily declined. This is not surprising in view of the fact that in 1903, according to the official reports, 855 canneries were operated on the coast, and that, as a Canadian commissioner admitted, the canneries can legally use almost everything in the form of a lobster which the fisherman catches.

In general, both in the states and in the provinces, reports of an increased yield of the fishery should be construed as an evidence of decline, for it can be shown that the greater yield is due to one or more of the following functions: increase in the number of traps and efficiency of the gear, in the number of fishermen, in the time of the fishing season, and in the area of the territory covered. While the number of lobsters caught may increase, the size and weight of the individual animals steadily diminish.

The tendency in past and present legislation has been to protect classes 1 and 2 as named above, in addition to female lobsters with eggs attached to the body. This is accurately described as protection of the young, and partial protection of the adult. It may sound very well, but weak spots appear upon a closer analysis. Class 1, the beginning of the series, in the course of nature must be recruited mainly by class 3, that is, from eggs of the largest producing adults, and by the very class which under present conditions is being wiped out. This policy shifts the duty of maintaining the first class upon class 2—or upon the small producers—a task which it is theoretically unable to bear, as well as practically incompetent to sustain, if one can draw any conclusions from the reports of the fisheries. No doubt there are many experienced persons who are ready to maintain that the present laws are good enough when properly enforced, but there is no way of getting over this grave defect.

The decline in the lobster fisheries is clearly due to the fact that more lobsters are being annually destroyed than are being reared in the course of nature. You can not get lobsters without eggs, and the egg-producers belong chiefly to class 3. It is further said that the protection of the female in spawn should remedy this defect. In reply to this we have to consider the fact, which I have demonstrated beyond reasonable doubt,[5] that the female lobster lays her eggs but once in two years. Consequently we should not expect to find more than one half of this class with external

[5] 'The Reproductive Period in the Lobster,' U. S. Fish Commission Bulletin, 1901.

eggs at any given time. This at once reduces the protection designed by such a law by one half, and the other half shrinks almost to the vanishing point, since between the climax of the period of hatching (June 15) and that of the spawning period (August 1) there is an interval of about six weeks when the majority of all adult females are without eggs, whether old or new, and therefore derive no benefit from such laws. Nor is it possible to ignore the fact that it is an easy matter for any fisherman to strip off the eggs from the female, and place her among his 'counters.'

In dealing with all such questions every one should avoid the common error of assuming that because any animal produces a large number of eggs, there must be a large number of adults reared from those eggs. This form of egregious logic is altogether too common among fish culturists in both England and America. On the contrary, the teachings of biology compel us to draw a very different conclusion. As I have elsewhere pointed out, the essential question—*what is the ratio between the number of eggs hatched and the number of young reared,* is strangely neglected.

An egg represents a chance of individual survival, and where the chance of survival is slight, the number of chances is increased. Vast numbers of eggs invariably mean certain destruction to all but a remnant of the host. I have also shown that a survival of two lobsters in every 10,000 hatched would be a large allowance, two in twenty or thirty thousand being, without doubt, nearer the truth. This further fortifies the conclusion that the vast numbers of eggs required to recuperate the first class can not be expected from the second class, but only from a permanently protected body of adults in full reproductive vigor. When the adults are permanently protected they form a growing class, since they will constantly receive as recruits all those animals which successfully run the gauntlet beyond the prescribed limit.

Those who object to a change of policy and to the adoption of method 2 given above might affirm that if class 3 has been practically exterminated, and if we proceed to wipe out class 2, soon there will be no more lobsters. This may be a serious objection, but on general principles we are assured that the change ought to be made, and made generally wherever the lobster is trapped; the sooner it is done, the better. No doubt if the legal length of the lobster were reduced from ten and one half inches to nine inches, the market supply would be increased for a number of years, and this might be followed by a stringency, but there would be a growing protected class at work all the time, and this would be bound to tell favorably in the end.

Many fishermen, accustomed for a lifetime to look upon the larger lobsters as their legitimate prey, would doubtless rebel against what might seem to them as opposed both to nature and to their own interests, but this would settle itself in course of time. Certain changes would be necessary in the construction of traps—in limiting the size of the funnel or the distance between the slats—but these would not entail serious expense.

To apply the principle of protecting the adult I should favor fixing the limits of length between which it would be legal to sell and destroy lobsters at eight to ten inches, permanently protecting all above and below these sizes. It might be an easier step from present conditions to set these limits between the nine- and ten-and-one-half-inch stages, which I am informed by Dr. Field is the plan favored by the department of fisheries and game in Massachusetts. This is not a vital matter so long as the principle of protecting the adult is maintained, and this is best done

by placing the bar close to the average period of beginning sexual maturity, or approximately at the ten or ten-and-one-half-inch length.

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WESTERN RESERVE UNIVERSITY,
February 12, 1906.

SCIENTIFIC BOOKS.

Congress of Arts and Science, Universal Exposition, St. Louis, 1904. Edited by HOWARD J. ROGERS, A.M., LL.D., Director of Congresses. Vol. I., Philosophy and Mathematics. Boston and New York, Houghton, Mifflin and Co. 1905. Pp. ix + 626.

On account of its comprehensiveness of plan, the large attendance of foreign scholars of the first eminence, and the picturesqueness (in several senses) of its attendant circumstances, the Congress of Arts and Science of the St. Louis Exposition was doubtless the most memorable and impressive scientific gathering ever held in America—as it was certainly the most creditable and original thing connected with the exposition. The more permanently valuable of its results will come less from the preservation of the papers read than from the stimulating influence of the actual assembling of so many great specialists for the comparison of methods and conclusions; from the informal discussions of workers in kindred fields, over restaurant tables or in the barracks where so much learning was housed in the midst of amateur soldiers, flying-machines and blanket-Indians; from the closer acquaintance brought about between scholars of a dozen different nations; and from the manner in which the congress brought home to the consciousness of a part of the world not hitherto adequately awake to such ideas the dignity of productive research, its central place amongst the functions of universities, and the primacy of its office in relation to all the work of modern civilization and to the increase of all forms of human power and wealth. For all this American men of science are in no small measure under obligations to all concerned in the organization and management of the congress—especially to the officials of the exposition, to the exposition's committee on congresses, to the boards responsible for the determination of the plan and scope of the congress, and to the foreign scholars who entered into the plan, often at considerable sacrifice of personal comfort and convenience. Much mention of personalities would be invidious; but it appears that the most distinctive features of this congress are to be credited to Mr. F. J. V. Skiff, director of exhibits, who insisted 'to the executive committee of the exposition that the congress work stand for something more than an unrelated series of independent gatherings,' and induced the committee to appropriate a sum sufficient to make practicable a project so extensive; to the late Mr. F. W. Holls, who suggested the idea of selecting and remunerating the speakers; and to Professor Münsterberg, whose imagination conceived the detailed plan finally adopted, and whose energy provided much of the driving power that made it possible to carry the plan through.

The present volume, the first of eight, contains a large amount of prefatory matter: a history of the congress by the editor of the series, Dr. H. J. Rogers; a paper on 'The Scientific Plan of the Congress' by Professor Münsterberg; and the eloquent opening address of the president, Dr. Simon Newcomb, on 'The Evolution of the Scientific Investigator.' Then follow the proceedings of 'Division A' of the congress—sixteen papers in philosophy and eight in mathematics—covering the field of what is called 'Normative Science.'

Münsterberg's classification of the sciences for the purposes of the congress has already been pretty widely criticized. No imaginable scheme of arrangement could fail to have its own special disadvantages. But there undeniably seems to be a supererogatory amount of perversity, and a needless sacrifice of practical convenience and naturalness of connection, in an arrangement which, *e. g.*, widely separates esthetics from psychology, theoretical from experimental physics, the philosophy from the history of religion, while bringing an edifying but rather preachy exposition of

Carlyle's 'Gospel of Work' into close proximity with a disquisition on 'The Theory of Invariants of Quadratic Differential Quantics.' Moreover, the scheme, with its uniform recurrence of 'divisions,' 'departments' and 'sections,' has an undue *a priori* rigidity, and does not properly take account of the actual contemporary interlacings of the problems of different sciences. The congress would probably have been more fruitful if the metaphor chosen to express its purpose had been, not the unification, but the cross-fertilization, of the sciences. In that case, perhaps, a greater proportion of the participants would have been at pains to make themselves intelligible and directly serviceable to men in other though not alien specialties; and we might have had a useful series of indications of just the light that workers in each field most need to have thrown upon their problems by workers in other fields. As it is, the 'unity of knowledge' sometimes shows only in a pretty abstract sense; and now and then the 'unification of the sciences' seems to owe more to the bookbinder than to the philosopher.

Concerning the propriety of grouping philosophy and mathematics together as 'normative sciences' much might be said; but the arrangement at all events serves to bring into clearer relief one of the real tendencies of the moment: the disposition to merge logic, metaphysics and mathematics together in a more fundamental science, a morphology of the primary formal concepts, which shall yield a new logic of relations. To-day—in the opinion of an influential group of thinkers, both philosophers and mathematicians—as at the beginning of the seventeenth century, philosophy is to be revivified by a transfusion of blood with mathematics; and mathematics is to be made more simple, more clear and more fruitful than ever before. As the subject is a favorite one with Professor Royce, he naturally improved the occasion, in his general address on the field of the whole 'division,' to insist upon the epoch-making significance of this new mathematical logic, and especially of the work of Kempe (which later is again set forth by Bôcher). It is an evidence of the strength of this tendency that the names of certain protagonists of the movement, Dedekind, Weierstrass, Cantor, Peirce, Peano, recur throughout the volume with greater apparent frequency than the name of Kant. It is of interest also to note that, partly because of this and partly because of other tendencies of contemporary thought, Leibniz, 'the first and greatest of German philosophers'—as he is called in Professor A. E. Taylor's very interesting paper—is enjoying a notable revival, much at Kant's expense. The signs of this appear alike in the papers of Royce, Taylor and Howison. This inclination to go 'back of Kant'—whose reputation has long been chiefly an obstruction to the progress of logic and metaphysics—is, so far as it goes, an encouraging symptom. There are those, however—and the present reviewer is among them—who find in much of the new mathematics only a straining of the concepts of ordinal arrangement and of correspondence into logical functions for which they are not fitted; who do not make out how, after all, the concepts of quantity and number can be reduced to anything else; who suspect the antinomies to be one of Kant's really sound contributions to logic; and who, in any case, can not share Royce's confidence in the direct serviceableness of the new logic of relations in the more concrete branches of philosophy. These, however, are too large matters to be argued out here. In emphasizing the tendency in question, the present volume at any rate gives a true picture of one striking feature of the contemporary situation. But another not less conspicuous tendency of the period—that known as pragmatism—is hardly so well represented. But for two or three brief references by writers unfriendly to the doctrine, no reader of this collection of papers would guess that pragmatism is the theme which, above all, fills our philosophical journals with controversy.

Of the two general papers in philosophy, Professor Howison's, on 'Fundamental Concepts and Methods' is only a torso. The comprehensive survey promised in the introduction does not appear; the part printed consists

chiefly in a fresh exposition of the author's own well-known system of pluralistic idealism —an exposition more technical and at points more thorough than any of the earlier ones. In view of Professor Howison's association, a generation ago, with the St. Louis group of philosophers, who did so much to introduce the German philosophical tradition into America, a certain historic appropriateness attaches to his place on this occasion as the first of the special representatives of philosophy and as the spokesman of a new argument which seeks to utilize the Kantian and the Hegelian logic to reestablish the Leibnitzian monadology. The other 'departmental' paper —one of the longest in the volume—by Professor Ladd, on the development of philosophy in the past century, is disappointing. The theme was a most alluring one; nothing could be more interesting than a review of the genesis and gradual growth and ramification of the several new fundamental concepts and presuppositions which were chiefly the discoveries of nineteenth-century thought—the idea of evolution, in its several phases, the invention of the philosophy of history and of the historical and genetic fashion of dealing with all problems, the manifold applications of the idea of relativity, the vicissitudes of the eighteenth century's favorite 'principle of contradiction' in subsequent logic and metaphysics, etc. But Ladd's treatment is pretty conventional, and, but for a few inconclusive generalities about the relations of philosophy and the sciences, consists largely in a dry catalogue of philosophers and their tendencies. Nor is the catalogue entirely accurate. It is, *e. g.*, misleading to speak of Reinhold as "rejecting Kant's arbitrary and self-contradictory 'thing-in-itself.'" Though the *Ding-an-sich* has a rather odd status in that system, it is nearer the truth to say with Falckenberg that Reinhold 'changed the thing-in-itself from a problematical negative, merely limiting concept, into a positive element of doctrine.' The summary in which F. Schlegel is disposed of is true only of his first period. Such figures as Lamennais, J. de Maistre—the great representative of the extreme reaction against the spirit of the Aufklärung—and Dühring, go unmentioned, while room is found for such names as Whedon, Hazard, Day and Tappan. The portrayal of the contemporary situation in philosophy is indefinite and inadequate.

Eight of the most important papers—those of A. E. Taylor (metaphysics), Hammond (logic), Woodbridge (logic), Ostwald (theory of science), Erdmann (validity of the causal law), M. Bôcher (mathematics) and Boltzmann (applied mathematics)—though scattered through different sections, form a connected group dealing with essentially the same topic—logic or epistemology. It is a pity that the program did not explicitly provide in advance for a single many-sided discussion of the logical foundations of the sciences, by the representatives of a number of distinct disciplines; here is a case where the mechanical uniformity of the scheme of the congress defeated its own purpose. But even as it is, these papers, read together, present an instructively diversified array of reasoning upon the same set of problems—the relation of logic to psychology, to metaphysics, to mathematics, the connection of the formal and the empirical elements in knowledge, the existence of intuitive or necessary truths, the ultimate criterion of validity in inference, the relation of the judgment to the 'transcendent object.' The result seems to show a general need of a better digestion of the work of the epistemological century—the eighteenth. For much that is ostensibly novel in the views presented seems due less to a real transcending of earlier positions than to a forgetting or an imperfect consideration of them. The question of the existence of 'necessary' truths and their relation to experience (a question, surely, that is capable of clear logical determination) still evokes a sharp conflict of opinions. Taylor declares that recent mathematical logic has only the more clearly shown the reality of self-evident principles and their primacy in knowledge, though it has also shown them to be reducible to a small number. Erdmann, in a similar spirit, observes that 'the assertion of modern scientific empiricism . . . that there is no

such thing as necessity of thought, goes altogether too far.' Bôcher takes a middle position, apparently holding to the validity of the criterion of mental necessity or ultimate self-evidence, as such, but doubting whether we can at any given time be sure that we can apply that principle to any specified proposition:

> We must remember, when we are tempted to put implicit confidence in certain fundamental logical principles, that . . . no very great weight can be attached to the mere fact that these principles appeal to us as obviously true; for other modes of reasoning which are now universally recognized as faulty have appealed in just this way to the greatest minds of the past.

Ostwald, speaking of the conclusion that if B follows A and C follows B in any well-ordered series, then C comes after A, says:

> The correctness and validity of this proposition seems to us beyond all doubt. But this is only a result of the fact that we are able to demonstrate it very easily in countless single cases, and have so demonstrated it. . . . To call such a proposition, however, a necessity of thinking does not appear to me correct. . . . To base the proof for the correctness of a proposition upon the impossibility of thinking its opposite is an impossible undertaking, because every kind of nonsense can be thought.

And Boltzmann deprecates an 'immoderate trust in the so-called laws of thought':

> Our problem cannot be to quote [*sic* the translator] facts before the judgment seat of our laws of thought, but to fit our mental representations to the facts.

Yet, somewhat oddly, Boltzmann is (in the same paragraph) sure that

> in facts there can be no contradictions. As soon as contradictions seems unavoidable we must test, extend and modify that which we call laws of thought, but which are [*sic*] only inherited, customary representations, preserved for æons for the description of practical needs.

As the requirement of non-contradiction is itself commonly understood to be nothing but the most fundamental of the laws of thought, the paragraph seems to show that contradictions are at any rate possible in the reasonings of a great physicist—when he turns aside into epistemology. The whole discussion of the question shows an undue amount of mental confusion and divergence of view, which it ought to be possible to get rid of, if philosophers and men of science would generally agree to study the history of philosophy understandingly and then 'get together' for an open-minded, patient, Socratic examination of their own meanings and of one another's views.

On the relation of logic to psychology, Taylor, Hammond and Woodbridge substantially agree in—I can not but think—misapprehending the matter. All three, while recognizing obvious points of contact, insist that (in Hammond's phrase) 'the essence of the logical problem is not touched by psychology, and should not be mixed up with it,' since psychology merely *describes* judgment and other mental processes, while logic inquires concerning *truth* in judgments. 'The psychological laws of the formation of concepts and beliefs are exemplified equally in the discovery and propagation of truth and of error,' says Taylor. But surely the only verifiable test of an absolutely true judgment (if there be such a thing) is the subjective fact that I can neither believe nor conceive its opposite; or of a probable judgment, that I find no adequate consideration which impels me to believe its opposite. At any given moment of inquiry, verifiable truth can, for anybody, only mean unescapable belief; probability can only mean belief conformable to preponderating, experience-engendered mental ease and habit. And the determination of the general sort of mental content in the presence of which such necessities of conception or deeply-rooted preferences of belief arise is certainly nothing but a question of introspective psychology. A normative principle can only be a way of stating a peculiar kind of descriptive fact, *viz.*, a necessary (and supposably universal) judgment-reaction exhibited by the mind in the presence of certain carefully analyzed meanings or ideational content. This was not unfamiliar to Locke or to Hume or to the Leibnitzians; but it seems of late to be too little considered. So, again, Woodbridge's vigorous and well-written argu-

ment for the realistic implications intrinsic in the judgment as such seems, after all, curiously like a mere relapse into a pre-Cartesian, even a pre-Protagorean, dogmatism. Doubtless a cognitive process purports to be 'connected with something other than itself,' and the truths which thought thinks are meant to be 'true, not about thought, but about things.' But it is also a peculiarity of the mind that it has the power of self-consciousness, and so is capable of doubting its own success in achieving this 'transcendent reference.' Such a self-conscious 'going-behind' the immediate content of consciousness, such a distinguishing of the thought-process from its potential object, necessarily supervenes in the history of philosophy and in any thoroughgoing reflection by the individual; and for any modern logician or metaphysician this reflective situation is already presupposed. The implications of the proposition that man is a self-conscious animal, Woodbridge hardly seems to have sufficiently considered.

At a moment when a renascence of realism is in fashion among metaphysicians—Dr. W. P. Montague even contending, in one of the shorter papers here printed, for the physical reality of the secondary qualities—it is interesting to turn to Poincaré's remarkable essay on the present condition of theoretical physics. He exhibits—in a fashion that will seem paradoxical enough to physicists of an older school—all the working principles which physics has long employed, as now subsisting in a very problematical and parlous state, and the concepts of matter and energy as surviving only in a singularly eviscerated form. The uncertainty and provisionality which are thus revealed in the theoretical foundations of the most fundamental of the physical sciences, by one who is perhaps its most eminent living representative, make this paper a noteworthy document in the history of science.

Erdmann's new rehabilitation of the concept of necessary causality appears in a rather bafflingly unidiomatic translation; but so far as one can follow the argument, it does not seem likely to render obsolete Ostwald's remark in the immediately preceding paper, that "all attempts to prove the general validity of the law of causality have failed, and there has remained only the indication that without this law we should feel an unbearable uncertainty in reference to the world." Erdmann's reasoning, however, is (though distantly related to the argument of Kant's 'Second Analogy of Experience'), original and *gedankenreich*, and it would be profitable to attempt an analytical discussion of it; but the paper is the longest of the series, and a commensurate treatment of it here is forbidden by considerations of space. Like considerations make it necessary to mention a number of the more specialized papers only by title: those of Ormond on 'Present Problems of Metaphysics'; of Pfleiderer and Troeltsch on the 'Philosophy of Religion'; of Sorley and Hensel on 'Ethics'; of H. R. Marshall and Dessoir on 'Esthetics'; of Pierpont on the 'History of Mathematics in the Nineteenth Century'; of Picard and Maschke on 'Algebra and Analysis'; of Darboux and Kasner on 'Geometry.' As has been sufficiently shown, the volume covers a very wide and very mixed field. The selection of these last-named papers for so brief mention is not due to any lack of interest and value on the part of most of them; it is rather due, partly to the limits of the province of this journal, and partly to the limitations of the present reviewer. Those who attended the sessions of the congress will remember that a number of the 'ten-minute papers' were by no means the least profitable part of the proceedings. Of these a few in philosophy, but none in mathematics, are printed—in each case in abridged form. The volume is not free from bad misprints; and most of the translations from French and German (that of Dessoir's paper, by Miss E. D. Puffer, is one exception) seem to be hasty renderings into that unknown tongue which only translators employ.

ARTHUR O. LOVEJOY.

The Eolithic Problem—Evidences of a Rude Industry Antedating the Paleolithic. By GEORGE GRANT MACCURDY.[1]

[1] *American Anthropologist*, N. S., Vol. 7, pp. 425–479, with five half tone plates reproduced

Within the last decades some of the principal questions regarding the Paleolithic stage in the evolution of man have come to be considered on a fair way to settlement, and the frontier of investigation in prehistoric anthropology has been pushed back into epochs representing the early Quaternary and the Tertiary. Some of the more important problems now under discussion concern the pre-Paleolithic or Eolithic stage and its culture. To these problems relating to the earliest culture of incipient man great interest attaches, and Dr. MacCurdy has materially assisted in making them understood in this country by presenting a clear and admirably constructed paper discussing the present stage of investigation in this field. He has taken the direct route to knowledge by visiting the original European localities and collections in company with investigators who have studied them, and his opinions are those of an unprejudiced observer with the original materials immediately before him. The paper includes an account of the early discoveries, special discussions of the finds in England and Belgium, a chronology of the stone age, and a very useful bibliography of the subject.

Technically, the Eolithic problem concerns the existence in Europe of implement-making and implement-using primates in periods antedating that of the Chellean or early Paleolithic industry. The time of the Chellean industry, or of the beginning of the Paleolithic, is not generally supposed to date back as far as the beginning of Quaternary time. The industry of this epoch is commonly acknowledged to represent a grade of development in implement making too advanced to be considered as the first stage. The stage of Eolithic man represents the epoch of beginnings, in which the first use was made of primitive implements. It is described as commencing at least as early as Miocene time, and extending upward into the early Quaternary.

The industry of Puy-Courny in France represents the late Miocene; the industry of the Chalk Plateau in the south of England, so fully discussed by Prestwitch and others, is held to be Pliocene. Other industries of France and England are referred to the late Pliocene. The numerous occurrences in Belgium to which Rutot has devoted himself are early Quaternary.

In a study of the implement-like objects attributed to the work of primitive man-like forms living in the earlier divisions of the Eolithic epoch great difficulties are necessarily met. The first implements were evidently unmodified natural objects. If selected, they were chosen because their original form was more suitable for the purpose in view than that of other objects. The first artifacts were probably unintentionally chipped by use, and this class of objects grades into that showing intentional modification of form. The series leads then from the typical implement to the unmodified natural object, and considerably before the beginning is reached we arrive at a point where it is almost impossible to determine whether or not one is dealing with artifacts.

Having seen a little of the original localities and collections examined by Dr. MacCurdy, it has appeared to the writer that the Eolithic question is really rather sharply divided. The problem of the Belgian Eolithic flints of early Quaternary age seems hardly the same question as that relating to the Pliocene eoliths of the Chalk Plateau in England, or that of the French specimens from the Miocene of Puy-Courny. As is shown by Dr. MacCurdy, the Belgian Eolithic remains, to which he attaches the greatest importance, exhibit in many cases almost undeniable evidence of intentional modification by man. They belong moreover to a period not far antedating the industry of the Chellean epoch, and are not so far removed from the present but that a paleontologist might conceive of the type of primate which made them as existing up to the present day without radical physical changes. On the other hand, the age of the older deposits representing the earlier portion of the Eolithic epoch is so great, that to any one acquainted with the rapid changes of mammalian types in time, it

from photographs of eoliths, and six text figures illustrating the geological relations of implement-bearing beds.

is difficult to conceive of a form closely related to recent man as extending back to this period. The most that we could imagine would be that the place of man was occupied by some form not higher than the Javan *Pithecanthropus,* and possibly considerably lower than that type, and a question naturally arises as to whether a primate of this stage of evolution would or could make use of implements.

In the case of the eoliths of the Kent Plateau, Dr. MacCurdy has produced evidence which seems to favor intentional modification of form. On the other hand, M. Boule in a recent article[2] has figured and described most remarkable flint forms resembling eoliths, but produced by the impact on each other of numerous flints carried about in swiftly running water at a cement factory. In such a case as this, in which from the very nature of the problem the discrimination between natural and artificial becomes increasingly difficult, it would appear that other evidence must be called in before we can reach definite conclusions. Apparently the ultimate decision concerning many of the most important points relating to the very early history of man must be determined by purely paleontological observations upon his skeletal remains, and the European record of these is as yet practically a blank for the Eolithic epoch. We shall, however, always obtain a large part of our information concerning early man from studies of the industries which represent him.

In whatever way the question of European Pliocene and Miocene man is finally settled, the present discussion is furnishing the occasion for considerable contributions to our knowledge of the origin and distinctive characters of flaked flints both natural and artificial, and will lead to a much better understanding of this side of the problem. Certainly no possible line of investigation which can furnish us information concerning the earliest man-like types should be neglected. Whether or not we are willing to agree with the investigators in all their conclusions in this particular case, we must certainly commend the earnest and painstaking effort which is being made to come to a clear understanding regarding the significance of the interesting materials now under consideration.

JOHN C. MERRIAM.

[2] M. Boule, 'L'Origine des Eolithes,' *L'Anthropologie,* 1905, T. 16, No. 3, pp. 257–267.

SCIENTIFIC JOURNALS AND ARTICLES.

The American Naturalist for March contains 'Notes on Reptiles and Batrachians of Pennsylvania, New Jersey and Delaware,' by Witmer Stone; 'Anatomy of Acmæa testudinalis Muller, Part I., Introductory Material —External Anatomy,' by M. A. Willcox; 'Affinities of Certain Cretaceous Plant Remains commonly referred to the Genera Dammara and Brachyphyllum,' by A. Hollick and E. C. Jeffrey; 'A New Pycnogonoid from the Bahamas,' by L. J. Cole; and 'Additional Notes on Bahama Snakes,' by T. Barbour.

Bird-Lore for March–April has a well-illustrated article by Herbert K. Job, entitled 'Some Bird Notes from the Magdalens,' 'A Familiar Sparrow Hawk,' by N. C. Brown, and 'Legs and Feet of Birds,' by C. William Beebe, showing their many modifications to adapt them for various uses. Under the section 'For Teachers and Students' we have the fifteenth paper on 'The Migration of Warblers,' by W. W. Cooke, and a 'Brief General Classification of the Songs of Eastern North American Wood Warblers,' by Gerald H. Thayer. In the Audubon Societies is noted the recent unanimous decision by the court of appeals that the sale of foreign game may be prohibited during the close season for similar native species. The Educational Leaflet is devoted to the belted kingfisher and includes a fine colored plate.

The Museums Journal of Great Britain for February contains the program for the July meeting of the Museums Association, which will be held at Bristol. There is an article on the 'Future of Museums,' by H. Bolton, which deals with the relations of provincial to government museums, a phase of museum administration that does not apply to the United States. 'Museums and Private Col-

lections,' by S. L. Moseley, shows how much harm may be wrought by the private collector and pleads for a more public spirit. The number is specially rich in notes on art museums and records the 'discovery' of a number of paintings by Turner in the cellars of the National Gallery.

The Museum News of the Brooklyn Institute for April contains articles on 'Zuni Pottery-making,' the 'Great Anteater' and 'The Care of an Aquarium,' besides numerous notes relating to the collections and libraries of the museums.

The Bulletin of the College of Charleston Museum has articles on the 'Birds of the Coast Region of South Carolina' and a synopsis of the museum lecture on typhoid fever.

SOCIETIES AND ACADEMIES.

THE NATIONAL ACADEMY OF SCIENCES.

THE regular annual session of the National Academy of Sciences was held in Washington, April 16 to 18, inclusive.

The following members were present during the session: Messrs. Abbot, Agassiz, Becker, Billings, Boss, Brewer, Brush, Campbell, Cattell, Chittenden, Crafts, Dall, Dutton, Emmons, Gill, Hague, Hale, Holmes, Howell, Merriam, Morley, Morse, Newcomb, Noyes, Osborn, Peirce, Pupin, Remsen, Trelease, Walcott, Webster, Welch, Wells and Woodward.

The following new members were elected: Benjamin O. Peirce, Cambridge, Mass.; William B. Scott, Princeton, N. J.; Josiah Royce, Cambridge, Mass.

Professor Wilhelm Ostwald, of Leipzig, and Professor H. A. Lorentz, of Leiden, were elected foreign associates.

Messrs. Billings, Chittenden, Hale, Osborn, Welch and Woodward were reelected members of the council for one year.

The Draper medal was presented to Mr. W. W. Campbell at a dinner given by Mr. Alexander Agassiz at the New Willard Hotel on Tuesday evening, April 17.

The following program was presented:

J. McK. CATTELL: 'The Distribution of American Men of Science.'

C. S. PEIRCE: 'Recent Developments of Existential Graphs and their Consequences for Logic.'

THEO. HOLM: 'Commelinaceæ. Morphological and Anatomical Studies of the Vegetative Organs of Some North and Central American Species.' (Presented by Theo. Gill.)

A. AGASSIZ and H. L. CLARK: 'On the Classification of the Cidaridæ.'

THEO. GILL: 'Interference of Oviposition of a Sargasso Fish with a Flying Fish.'

H. F. OSBORN: 'Faunal and Geological Succession in Eocene and Oligocene Basins of Rocky Mountain Region.'

W. J. SINCLAIR: 'Volcanic Ash in the Bridger Beds of Wyoming.' (Presented by H. F. Osborn.)

C. E. DUTTON: 'Radioactivity and Volcanoes.'

C. D. WALCOTT: 'Cambrian Faunas of China' (with lantern illustrations).

GEORGE E. HALE: 'Recent Solar Investigations' (with lantern illustrations).

W. W. CAMPBELL and C. D. PERRINE: 'Some Recent Solar Eclipse Results.'

M. I. PUPIN: 'Feeble Rapidly Alternating Magnetization of Iron.'

J. M. CRAFTS: 'Primary Standards for Temperature Measurements between 100° and 350°.'

ASAPH HALL: 'Biographical Memoir of Admiral John Rodgers.'

W. M. DAVIS: 'Biographical Memoir of George P. Marsh.'

THEO. GILL: 'The Life History of Pterophryne.'

SOCIETY FOR EXPERIMENTAL BIOLOGY AND MEDICINE.

THE fifteenth meeting of the Society for Experimental Biology and Medicine was held in the Physiological Laboratory of the New York University and Bellevue Hospital Medical College on Wednesday evening, February 21, 1906. The president, Edmund B. Wilson, was in the chair.

Members Present.—Auer, Beebe, Brooks, Calkins, Emerson, Field, Gies, L. Loeb,[1] Lusk, A. R. Mandel, J. A. Mandel, Meltzer, W. G. MacCallum,[1] Murlin, Opie, Park, Richards, Salant, Shaffer, Sherman, Torrey, Wallace, Wilson, Wolf.

Members Elected.—Walter R. Brinckerhoff, Warren P. Lombard, B. T. Terry, E. E. Tyzzer.

Officers Elected.—President, Simon Flexner; vice-president, E. K. Dunham; librarian, Gra-

[1] Non-resident.

ham Lusk; treasurer, Gary N. Calkins; secretary, William J. Gies.

Abstracts of Reports of Original Investigations.[2]

On the Intermediary Metabolism of Lactic Acid: A. R. MANDEL and GRAHAM LUSK.

Administration of phlorhizin to a dog poisoned with phosphorus causes excretion of dextrose, the mother substance of lactic acid, and the latter then disappears from the blood and urine. On the other hand, *d*-lactic acid (Kahlbaum), when given to a diabetic dog, may be completely converted into dextrose.

The Primary Factor in Thrombosis after Injury to the Blood Vessels: LEO LOEB.

The author has observed that in invertebrates as well as in vertebrates an agglutination of blood-cells or of blood plates may take place around foreign bodies or at the place of injury of the vessel wall. The formation of such agglutination thrombi was found to correspond to the clumping of the same cellular elements outside of the body, where the agglutination can take place without being accompanied by a coagulative process. This phenomenon is observed in vertebrate as well as in invertebrate blood.

In birds the injection of hirudin does not materially alter the readiness with which a thrombus is formed. In dogs, on the other hand, it is very probable that injections of hirudin delay or may sometimes prevent the formation of agglutination thrombi. The effect, however, is not directly due to the inhibition of the coagulation of the blood, but probably to changes in the blood which will have to be determined.

Granula and Ameboid Movements in the Blood-cells of Arthropods: LEO LOEB.

The author's investigations of the changes in freshly drawn blood of Limulus and other arthropods show that the fate of the granules of the blood-cells depends upon certain mechanical conditions and that the apparently spontaneous dissolution of cell granula can be inhibited, to a large degree, by preventing certain mechanical irritations of the cells. It seems probable that the ameboid movements, the spreading out of the cells and the dissolution of the granules are caused by certain metabolic changes which are induced in each instance by similar conditions.

On a Course in the Pathological Physiology of the Circulation, with Demonstrations of Tracings, Anatomical Specimens, Instruments, etc.: W. G. MACCALLUM.

The author described the course given by him during the past year in the new laboratory of experimental medicine at Johns Hopkins University. It was the aim of the course to reproduce experimentally such diseased conditions as are seen by medical students in the wards of the hospital, so that the diseases might be studied with the aid of any or all of the methods at the command of the physiologist and of the pathologist. The author's description and demonstrations made it evident that the object of the course has been attained with an unusual degree of success.

On the Blood-pressure Relations in Experimental Mitral Insufficiency and Stenosis, with Demonstrations of Tracings and Anatomical Specimens: W. G. MACCALLUM and R. D. MCCLURE.

Blood pressure was recorded in various portions of the circulatory system after mitral insufficiency had been produced by introducing a curved knife hook into the left auricular appendage and cutting some portion of the mitral valve. A systolic murmur could then be heard, which was especially loud over the auricle and along the pulmonary veins with usually a thrill felt over the auricle. The hypertrophy of the right ventricle was studied and discussed.

Mitral stenosis was produced by means of a clamp or by a coarse suture passed through the heart and about the mitral ring. The pressure is seen to rise very high in the pulmonary circulation, but, because of the smaller amount

[2] The abstracts presented in this account of the proceedings have been greatly condensed from abstracts given to the secretary by the authors themselves. The latter abstracts of the communications may be found in current numbers of *American Medicine* and the *New York Medical Journal.*

of blood to circulate there, it is lower throughout the systemic circulation.

Paramecium Aurelia and Mutation: Gary N. Calkins.

In March, 1905, a pair of conjugating *Paramecium caudatum* was isolated from a culture in an epidemic of conjugations. The ex-conjugates had all of the characteristics of *P. aurelia*. One died before many generations in culture, the other is still living and is now in the 346th generation. This one retained the characteristics of *P. aurelia* until about the 45th generation after conjugation, when it lapsed again into the *P. caudatum* form, with one micronucleus and other characteristics of *P. caudatum*. The latter characters are still maintained.

The observation indicates one of two things. Either, this is an interesting case of mutation of species, with lapse into the parent form after several generations, or the specific characteristics are inadequate and *P. caudatum* and *P. aurelia* are but variants of one species. The latter is the more reasonable hypothesis, and on grounds of priority the common forms of paramecium should be called *paramecium aurelia*.

Experiments with some Saline Purgatives given Subcutaneously: John Auer.

The author's experiments lead to the conclusions that the subcutaneous injection of sodium sulfate and sodium phosphate does not produce purgation in rabbits, and that the pendular movements of the small intestine are moderately increased.

The Effects of Extra Stimuli upon the Heart in the Several Stages of Block, together with a Theory of Heart Block: Joseph Erlanger. (Presented by S. J. Meltzer.)

The author's observations suggest the following theory of heart block: Clamping the auriculo-ventricular bundle reduces the efficieny of the cardiac impulses that reach the ventricles. With a certain degree of pressure the impulses become subminimal with respect to the irritability of the ventricles. Such an impulse would, therefore, fail to elicit a contraction of the ventricles. The next following auricular impulse would be no stronger than the preceding one, but in the interval the irritability of the ventricles has increased to the extent that the weakened auricular impulse now acts as an efficient stimulus. In this state of affairs the rhythm would be 2:1. A further reduction in the efficiency of the auricular impulse would give higher degrees of partial block and finally complete block. With this theory as a basis it becomes possible to explain all of the important phenomena of heart-block.

On the Nature of the Reflexes Controlling the Successive Movements in the Mechanism of Deglutition: S. J. Meltzer.

The experiments demonstrate that the function of deglutition is provided with two sets of reflex mechanisms. One mechanism has only one initial afferent impulse which travels within the center independently of any further aid from the esophagus; it is very sensitive to anesthesia and may be called a higher reflex. The other is a lower reflex, consisting of a chain of local reflexes which are very resistant to anesthesia.

The Enzymes of Inflammatory Exudates. A Study of the Enzymes concerned in Inflammation and their Relations to Various Types of Phagocytic Cells: Eugene L. Opie.

The phagocytic cells of an inflammatory exudate contain two enzymes. One of these ferments, characterized by its power to digest protein in an alkaline medium, is contained in the polynuclear leucocytes with fine granulation, and since it is derived from the bone marrow, may be designated *myelo-protease*. The second ferment characterized by its power to digest only in acid medium, in this respect resembling the autolytic ferments of other organs, is contained in the large mononuclear cells of the exudate and is increased in lymphatic glands adjacent to the seat of inflammation; it may be designated *lympho-protease*.

Experimental Myocarditis. A Study of the Histological Changes following Intravenous Injections of Adrenalin: Richard M. Pearce. (Presented by Eugene L. Opie.)

The author gave many detailed results of an elaborate study. He stated that there is little

evidence to support the theory of a direct toxic action of adrenalin on the heart muscle. Indeed, the limitation of degenerative lesions to the heart and larger blood vessels and their practical absence in all other tissues contraindicate a toxic action and point to some influence of a mechanical nature affecting these structures alone. That some of the more unusual forms of fibrous myocarditis in man which are difficult of explanation may be due to circulatory disturbances of the same general nature as those caused in rabbits by adrenalin can not be denied. It is to these only that the results of this experimental investigation appear to have any relation.

Stable and Detachable Agglutinogen of Typhoid Bacilli: B. H. BUXTON and J. C. TORREY.

By heating an emulsion of typhoid bacilli to 72° C. for half an hour a detachable agglutinum may be separated from the bacilli. This may be obtained in the filtrate after passage through a Berkefeld filter. Rabbits which have been inoculated on the one hand by this filtrate and on the other by the heated bacilli, which have been thoroughly washed, show specific differences in their serums, as regards agglutination. The animal inoculated with the washed bacilli or stable agglutinum, produces a serum which agglutinates normal typhoid bacilli very slowly and with the formation of fine clumps. In contrast to this, the filtrate containing only detachable agglutinum gives rise to serum which clumps normal typhoid bacilli rapidly and with the formation of large flocculi.

Absorption experiments, furthermore, show that the *s* or stable agglutinin and the *d* or detachable agglutinin are distinct in character.

It has also been determined that the substance in typhoid bacilli which gives rise to precipitins for filtrates of typhoid cultures is split off from the bacilli, together with the detachable agglutinums. The possibility suggests itself that the *d* agglutinin and the precipitin in a typhoid serum are identical.

The Effect of Alcohol on Hepatic Glycogenesis (Preliminary Communication): WILLIAM SALANT.

Thus far in his experiments the author has found that the administration of alcohol, even in relatively large doses, is without effect on glycogen metabolism in the livers of fasting rabbits.

The Viscosity of the Blood during Fever and After Injection of Phenylhydrazin: R. BURTON-OPITZ.

The viscosity was increased in these experiments, whereas the specific gravity was decreased. These results agree with the author's previous observations, to the effect that the viscosity and the specific gravity of blood may vary independently.

WILLIAM J. GIES,
Secretary.

THE BIOLOGICAL SOCIETY OF WASHINGTON.

THE 414th meeting was held March 17, 1906, with President Knowlton in the chair and 31 persons in attendance. Mr. J. W. Gidley presented the first paper, 'Evidence bearing on Tooth-Cusp Development, based on a Study of the Mesozoic Mammals.' A study of the Mesozoic mammal collection in the U. S. National Museum has led to some discoveries of importance bearing on the question of tooth-cusp homologies in the mammalian molars.

According to the tritubercular theory, as advocated by Osborn, the primary cone in the upper molars is always to be found on the inner or lingual side and is the homologue of the central cone in such forms as *Triconodon*. Against this theory Scott has shown, from paleontological research, that in the upper molariform premolars the primary cone is on the *outside*. M. F. Woodward has found from embryological studies of certain groups of insectivores that the main anterior *external* cusp is the first to develop, not only in the premolars, but in the molars as well, while the internal cone (*protocone*) is third in making its appearance.

Notwithstanding this opposition evidence, Osborn still supports the hypothesis of an internal position for the primary cone in the

upper molars, and as conclusive evidence of the correctness of this view has cited the upper molars of *Triconodon,* in which the main cone is central, and *Dryolestes* and other forms, in which he states the main cone is internal.

Owing doubtless to the incompleteness and minuteness of the teeth examined, Osborn was apparently led to error in observations, for instead of three cusps, one internal and two external, as stated by Osborn, the upper molars of *Dryolestes* have five distinct cusps, one internal, *three* external and one posterior median. This arrangement admits of a different interpretation of the cusp homologies. The three outer cusps supported by two fangs now appear to be homologous with the three main cusps and two fangs of *Triconodon,* the inner cusp being readily interpreted as a secondary or internal heel development. This view is strengthened by a third type, *Dicrocynodon,* in which the outer portion of the upper molar is very similar to that of *Dryolestes,* but the large inner cusp is totally different. In *Triconodon* a broken external cingulum and two incipient inner heel-like cusps preclude the probability, at least, of this form of molar ever passing to a typical triconodont stage through the outward shifting of its lateral cusps. These forms, therefore, apparently represent distinct types of molars separately derived from the simple cone, and *Triconodon* and *Dryolestes* do not represent successive stages in the development of the trituberculate molar, as supposed by Osborn.

Thus, the evidence of the Jurassic mammals apparently agrees with the embryological evidence and supports the 'premolar-analogy' theory, while it lends no support to the tritubercular theory in so far as it involves the position of the primary cone.

The second paper was by Mr. M. C. Marsh, on 'Hemoglobin Estimates and Blood Counts in Fishes in Health and Disease.' The species observed were the brook trout and rainbow trout. Apparently normal brook trout from the Au Sable River in Michigan varied widely in hemoglobin and no norm was established save a very broad one. Thirty-five specimens gave an average reading of 43 with the Dare hemoglobinometer, 100 representing normal human blood. The readings varied from 33 to 59. The hemoglobin of domesticated brook trout averaged 34 from 23 observations. Wild rainbow trout, represented by only two closely agreeing readings, were 92 in hemoglobin, while the same species domesticated averaged 54 from 19 observations. The chief conclusions of interest derived from these figures are that trout blood is lower in hemoglobin than human blood, that the brook trout, whether wild or domesticated, is considerably lower than the rainbow in the corresponding condition, and that domestication is attended with a considerable falling off in hemoglobin content. This latter fact is possibly correlated with the increased susceptibility to disease in domesticated fish. Such a correlation is more forcibly suggested by the comparative insusceptibility of rainbow trout, even in domestication, to a bacterial disease which under exactly the same conditions attacks readily the brook trout. The question is raised of the possibility of increasing the hemoglobin in the blood of the latter species by feeding iron salts, as in human medicine.

The wild brook trout has about one million red corpuscles per cubic millimeter and the number is not diminished in domestication. The rainbow trout domesticated has 1,487,000, being the average of eleven individuals, while a single observation of the wild rainbow showed 1,830,000.

Trout in fish-cultural ponds have occasionally true neoplasms of a malignant nature located in the region of the gills and causing a pronounced anemia. Ten brook trout thus afflicted had an average hemoglobin reading of 17. An apparently primary anemia in the young of this species has been observed, in some individuals so extreme that the gills in life were white. The red cells had fallen off greatly in number, the lowest count recorded being 38,000.

On the other hand, the most destructive epidemics of protozoan and bacterial infection in trout are not attended by any marked anemia.

Mr. Austin H. Clark read the last paper of the meeting, describing 'A Case of Melanism in West Indian Honey Creepers.'

M. C. MARSH,
Recording Secretary.

DISCUSSION AND CORRESPONDENCE.

DR. O. F. COOK'S CONCEPTION OF EVOLUTION.

IN SCIENCE, March 30, 1906, p. 506, Dr. O. F. Cook expresses the opinion that in the recent discussion of isolation as an evolutionary factor there is 'a need of a simple distinction,' and asserts that isolation does not play a part in evolution. A similar idea, that neither isolation nor natural selection nor mutation factors in evolution, had been maintained by him previously in a series of publications, the last of which is a paper printed by the Washington Academy of Sciences.[1]

This astonishing view should be carefully investigated and analyzed, for up to the present time every writer on evolutionary subjects, no matter what his standpoint, has taken it for granted that any of the factors introduced, if they are admitted at all, are admitted on the ground that they are factors cooperating in the general process called evolution. Dr. Cook, however, believes that isolation, natural selection, mutation, etc., have nothing to do with evolution, and that the last is a different process, due to 'causes resident in species.'

Looking more closely upon his views, it becomes evident that Dr. Cook's conception of 'evolution' is different from that of other writers, and, of course, the propriety of his criticism of the latter depends on the correctness of his new conception of evolution.

As every student of evolution knows, and as also Dr. Cook admits,[2] 'evolution,' as the word implies, was originally intended to characterize the whole process by which the organic world has been formed. According to the view of Linnæus, the organic world, as it now exists, divided up in species, was created so, and the number of existing species has remained permanent since their creation; according to Cuvier, a number of successive creations of species have taken place, each destroyed by a catastrophe. The 'theory of evolution' is opposed to the assumption of a permanency or stability, and introduces the view that the present organic world has developed out of preexisting forms, the former being evolved, or developed, or descended from the latter, and it admits the possibility of the splitting up of one species into two or more. Thus 'evolution' becomes a concept contrary to permanency or stability, and expresses the belief that organisms have reached their present state by degrees, by a change or transmutation, which they have undergone during the process of descent from their ancestors, connected with a differentiation. Since this theory has been proposed in order to explain the present condition of things, chiefly the separation of the organic world into a large number of species, the whole process of evolution has been called by Darwin 'origin of species,' and Darwin's theory is known as the 'theory of evolution,' or the 'theory of descent,' and the terms 'evolution,' 'descent,' 'development' have been used as synonyms.

But this is wrong, according to Dr. Cook. Already Darwin's phrase 'origin of species' (the 'species-origination box,' as Dr. Cook very elegantly calls it) does not include the factor of 'evolution,' for evolution is different from 'speciation,' or the making of species. Evolution is a 'process of organic change and development, universal and continuous'; it is a 'continuous progressive change'; it is the 'progressive development of organisms'; it is a 'process of change in species'; which means to say that it is characterized by a continuous *change* of the organisms, which becomes evident and visible by the fact that the descendants differ from their ancestors. This *change* observed in the organic world is paramount in Dr. Cook's conception of 'evolution'; he restricts this term thus, and uses it exclusively to express this fact. What happens later to the changed organisms through the action of natural selection, segregation, etc., is entirely

[1] O. F. Cook, 'The Vital Fabric of Descent,' *Proc. Wash. Acad. Sci.*, 7, March 19, 1906, p. 301 ff.

[2] O. F. Cook, 'Evolution not the Origin of Species,' *Pop. Sci. Mo.*, 64, 1904, p. 445.

outside of 'evolution,' and is another process, called by Dr. Cook 'speciation.' Both processes are connected only in so far as evolution furnishes the material for speciation.

This analysis shows at a glance that what Dr. Cook calls 'evolution' is in fact nothing but the well-known process of 'variation';[3] possibly it is only a special form of it, since according to Dr. Cook's statements, a progress or advance is implied in 'evolution.' Be this as it may, evolution in Dr. Cook's sense is certainly included in the old concept of variation, that is to say in the general and fundamental axiom of the Darwinian theory that organic beings, during the process of development, change or vary, that the descendants may differ from their ancestors, that a change of characters takes place during the phylogenetic development of organic forms.

Thus Dr. Cook's idea is *new* only in so far as he tries to restrict the original meaning of the term 'evolution.' In previous literature, 'evolution' includes *all* factors that contribute to the development of the organic world: it includes variation as well as inheritance, natural selection and segregation[4] and several others, which have not found universal recognition as independent processes. But now Dr. Cook tries to teach us that the word 'evolution' should be deprived of its general meaning, and should be used only in place of 'variation,' with a peculiar restriction.

It hardly seems advisable to accept this change of the meaning of a word used in the same sense by *all* previous writers. Although Dr. Cook feels the necessity of doing so, and in spite of his criticism of the 'chosen people of science' for their failure to see the propriety of this change, I for my part prefer to call the whole process of development of the organic world, from its beginning to its end, by the name of 'evolution,' which is synonym to 'development,' and also to 'origin of species,' 'descent,' and also to 'Darwinian theory.' Dr. Cook's 'simple distinction' between 'evolution' (= variation) and 'speciation' (= all other factors) is not simple at all, but highly confused and confusing, since the meaning of a well-established word is arbitrarily changed, without the slightest necessity (other terms being available). Thus I must positively decline to accept Dr. Cook's conception of 'evolution.'

To the disinclination of other men of science to accept the terminology suggested by Dr. Cook is apparently due his complaint that the 'very ungracious task to convince' them of the correctness of his position falls upon his shoulders. But there is no need for him to complain. *The distinction recommended has actually been made before,* and there have been other people who have conceived similar ideas, although different terms were used by them. I myself have emphasized in the article referred to by Dr. Cook,[5] that I regard isolation only as a factor *in species-making* (speciation), and have quoted a paper of mine,[6] where I have set forth my views in detail. Thus, five years before Dr. Cook's first publication on this subject,[7] I have 'perceived these elementary facts,' that there are not only 'two groups of phenomena belonging to entirely different categories,' but that there are *four* of them. *The first of them is variation,* which furnishes the material for the others, and must be taken for granted, no matter 'what an Irishman might say.' But this has not 'saved the writing' of Dr. Cook's papers, for he apparently has not taken the trouble to ascertain what my views are. Moreover, I do not claim, by any means, to be the only one who was able to 'perceive this elementary fact' that the origin of species is composed of several processes belonging to different categories, but I have always affirmed that already Darwin, in the 'Origin of Species' very properly distinguished them and discussed them, at least saw clearly *that there are different questions involved.* That Darwin has been misunderstood and misin-

[3] 'Evolution . . . is the journey of which individual variations are steps.' O. F. Cook, in *Pop. Sci. Mo.*, 64, 1904, p. 449.

[4] For particulars see *Proc. Am. Philos. Soc.*, 35, 1896, p. 188.

[5] Science, January 12, 1906, p. 71.

[6] *Proc. Am. Philos. Soc.*, 35, 1896, p. 175 ff.

[7] Science, 13, 1901, p. 969.

terpreted by those that have studied his writings, is to be regretted, but is excusable; that his views are judged upon without his works being read, as is sometimes the case, is inexcusable.

Aside from the above objection to Dr. Cook's use of the term 'evolution,' I wish to emphatically object to his idea of the 'actuating causes' of 'evolution' (or variation). He believes that they are not to be sought in the 'pressure of environment,'[9] but that they are 'inner' causes, supported by interbreeding.

This view is not new at all, indeed we may say that, by this time, *it is venerable on account of its antiquity,* for it is the view held by the earlier Weismannian school, which assumes that variation is due to inner causes (germinal variation, spontaneous variation, Keimvariation), aided by amphimixis (interbreeding). I have demonstrated[9] that this view, which, as it is proper to state, is not held any more by Weismann himself, is entirely illogical; but I do not see the necessity of repeating here my arguments for Dr. Cook's benefit. This much, however, may be said, that the assumption that only inner causes are 'actuating' in the production of variation, expressly excludes a class of causes which is absolutely necessary for every process in this world, namely the 'causæ efficientes.' That Dr. Cook has entirely forgotten what a 'causa efficiens' is is shown by the distinction he makes between *occasion* and the *true, actuating cause.*[10] But he may be excused on the ground that the discovery of the difference of these terms, and of the fact that what he calls *occasion,* is no *true cause,* is not his: it is a perpetuation or repetition of a blunder committed first by Weismann,[11] and by von Graff,[12] in making a distinction between *Bedingung* and *Ursache,* or *condition* and *cause.*

[9] C. H. Merriam, SCIENCE, February 16, 1906, p. 244.

[9] 'Ueber Keimvariation' in *Biolog. Centralblatt,* 18, 1898, p. 139 ff.

[10] *Proc. Wash. Acad. Sci.,* 1906, p. 305.

[11] 'Ueber Germinalselection,' 1896, p. 48, footnote 2.

[12] 'Zoology since Darwin' in *Ann. Rep. Smiths. Inst.,* 1896, p. 486.

Indeed, it is too bad that this discovery of Dr. Cook, that the *occasion* (or *condition*) is no *actuating cause,* can not stand in the face of philosophical criticism. For, if the *occasion* of Dr. Cook is the same thing that is called *causa efficiens* (*actuating cause*) by people trained in logic, then, of course, *external influences must be admitted as the causæ efficientes of variation.*

A. E. ORTMANN.
CARNEGIE MUSEUM, PITTSBURG, PA.
April 2, 1906.

THE DISTRIBUTION OF GOVERNMENT PUBLICATIONS.

To the Editor of Science: The letter on page 545 of SCIENCE for April 6, 1906, from Junius Henderson, of Boulder, Colo., relates to a subject that has always had a personal interest for me. I can never forget the advantages that I myself derived from the generosity of a father who enabled me to begin the accumulation of a scientific library. Equally advantageous have been the gratuitous publications of the government, and the comparatively cheap publications of scientific societies, as contrasted with the very high prices charged by many publishing firms for strictly technical scientific documents. It is to the best interests of our national government, our state governments and our endowed universities that they should, in every way possible, stimulate the publication and distribution of researches that, taken collectively, mark the steady progress of man in wresting her secrets from nature.

Perhaps to an equal degree is it the duty of the citizens, so far as is any way practicable, to stimulate the establishment of scientific and technical libraries in localities where they may be accessible to large numbers of students. The increase and diffusion of knowledge should not be left to the Smithsonian alone, or to the government, or to the university as an organization, but has become the duty of each individual scholar. Many men have considerable collections of valuable books that they should make accessible to students, rather than keep them locked up on their own shelves. I know of several who are

looking about for the best university or library in which to deposit their own scientific collections. If the wants of our universities and observatories and research stations could be fully made known, *through the columns of* SCIENCE, they would find a ready response on the part of individuals who have been profiting by the generous distribution of expensive volumes during many years past. Such volumes, whether published by the government or by societies, are, as it were, loaned in trust to past recipients, who, having benefited by them, should now in turn pass them on to others, rather than hoard them, or sell them as merchandise.

CLEVELAND ABBE.

THE MENTAL DEVELOPMENT OF INDIVIDUALS.

TO THE EDITOR OF SCIENCE: I wish to learn at what age, under what circumstances and to what extent people of different climes, races, civilizations and temperaments have changed their views as to whence we came, whither we go, and what we are here for. Any statement, elaborate or short, regarding an individual's mental development will be a welcome contribution to a proposed 'Natural History of the Thinker.' I have been obliged to thus appeal to my contemporaries because autobiographical documents so far extant do not yield enough accurate descriptions of the inner life. To illustrate my purpose, I beg to refer to my article on 'The Interpretation of a System from the Point of View of Developmental Psychology,' in the *Journal of Philosophy, Psychology and Scientific Methods,* February 15.

EDWIN TAUSCH.

OHIO UNIVERSITY, ATHENS, O.

SPECIAL ARTICLES.

QUARTZ GLASS.

PURE quartz when melted down to a glass has three properties which make it of immense value in the chemical and physical laboratory, and were it not for the technical difficulties attending its production, it would certainly displace ordinary glass wherever a transparent medium capable of withstanding heat is required. It expands less than one tenth as much as common glass when heated; it can be heated to 1,000° C. without softening; and finally, it transmits ultraviolet light freely.

It has not proved easy to make quartz glass, even in small quantities in the laboratory. Quartz is one of those peculiar minerals[1] which show no sharp melting temperature, but soften very gradually, and when pure, never become thin liquids, even at the temperature of the electric arc. Furthermore, quartz begins to vaporize rapidly in air at about the temperature of melting platinum, while it is still much too viscous to release the included bubbles. A mass of quartz fragments, when melted in air in the electric furnace, comes out resembling solidified sea-foam or volcanic pumice. It is quite opaque, dirty and useless for mechanical or optical purposes, and very persistent efforts in a number of laboratories have so far failed to produce a clear product except from single fragments treated individually. Small globules of glass can be obtained from single crystals, pieced together in the oxyhydrogen flame, and blown into thin quartz glass vessels such as are now in quite common use. Discs suitable for small lenses have also been obtained at Jena by heating small clear crystals with such rapidity as to produce a thin enclosing film of liquid before cracks develop in the body of the crystal, thereby prevénting the entrance and subsequent enclosure of air. It is, of course, plain that such devices can have but limited usefulness. We must somehow manage to melt larger masses of random fragments to a clear glass before the technical problem can be regarded as solved.

This problem is somewhat outside the proper scope of the Geophysical Laboratory, but our plant is perhaps better adapted to its solution than most others, and the demand for clear quartz glass is so general that it seemed best to spend a limited time in an effort to find the difficulty and to try to ascertain the direction in which the solution lies. No effort at refinement of method has yet been made.

[1] See Day and Allen, 'The Isomorphism and Thermal Properties of the Feldspars,' Publ. 31, Carnegie Institution.

When pure crystallized silica, either quartz or tridymite, is fused in graphite crucibles, there is no difficulty in obtaining quartz glass. The difficulty, as indicated above, is to free it from enclosed air. After a series of experiments at different temperatures, it became evident that there was no probability of obtaining clear glass by direct fusion at atmospheric pressure. It was, therefore, decided to study the effect of pressure upon the fusion of silica.

The experiments were conducted in a large bomb furnace under a pressure of 500 pounds of compressed air, the heat being supplied by passing an alternating current through the walls of a thin graphite box containing the quartz. The heating was at first carried to a much higher temperature than was necessary in an effort to reduce the viscosity sufficiently to release the air bubbles in the normal way, but the attempt failed entirely—the viscosity is but slightly diminished at the higher temperatures, and enough silica is reduced to discolor the mass with free silicon. The appearance of the product also clearly showed that gas was being generated at the hottest points in the retaining walls, and the large bubbles formed by the rapid expansion of this gas were always lined with free silicon. At the highest temperatures (above 2,500° C.), therefore, we not only did not get rid of the air bubbles enclosed in the glass, but introduced a new disturbing factor.

The next step was, of course, to reduce the temperature and lengthen the time of heating. This produced blocks of quartz glass which were quite transparent but which contained a great number of small included bubbles which could not be displaced even when the time of heating was extended over several hours. No large bubbles appeared, however, and no discoloration. An effort to explode the enclosed bubbles by turning on the compressed air before the heat was applied and then releasing the pressure while the material was still molten, also failed. The inflated material could not be brought back to a cake again.

After a number of attempts of this character, with slightly varied conditions of temperature and pressure, a charge was heated rapidly to a high temperature (considerably above 2,000°) before pressure was applied. After the quartz had begun to vaporize freely, it seemed reasonable to expect that the vapor would displace the air between the grains somewhat as mercury vapor is made to displace the air in filling thermometers. Compressed air was then quickly applied to compress the melt into a compact mass, the temperature lowered to the point where it had been found safe to work without discoloration, and held there for perhaps a half hour, after which the current was turned off and the pressure very gradually withdrawn. Plates of quartz glass $3 \times 5 \times \frac{1}{2}$ inches were produced under these conditions which were almost entirely free from bubbles, and only occasionally slightly stained by free silicon. The residual bubbles are very small, not more than $\frac{1}{2}$ mm. in diameter, and are not frequent enough (not more than two or three in a cubic centimeter) to interfere with the use of the glass for lenses, mirrors or other usual optical purposes. It is, furthermore, very probable that a little more skill in handling, such as could readily be obtained with longer experience, would get rid of even the few remaining bubbles.

Quartz glass is easily stained by very small quantities of other oxides when present as impurities. In particular, we found that as little as .3 of one per cent. of other oxides was sufficient to make the glass opaque and almost black. It is, therefore, absolutely necessary to start with very pure material, but it does not require to be clear. Pure cloudy quartz serves quite as well.

The volatility may be due to one of two causes: either the vapor pressure of liquid quartz is very great, or the carbon reduces the silica, with the formation of metallic silicon, which at once volatilizes and is subsequently reoxidized on passing into the surrounding atmosphere. This reaction, that is, the reduction to the metallic state and subsequent volatilization, is a very common one at these

temperatures, and has misled several investigators into interpreting the volatilization of the oxide to be due to its vapor pressure, when in fact the oxide, heated in the absence of carbon or reducing papers, shows little or no volatility. The volatilization of magnesium oxide is of this class.

Whether the pressure which is essential to the preparation of good quartz glass in quantity acts upon the vapor pressure of the silica, or whether it affects the reduction of the silica to the metal, has not yet been determined. It is not unlikely that both reactions occur, and that the narrow temperature limits within which we found it practicable to work, lie between the temperature of volatilization of the silica and that of its reduction by carbon. This question is not material to the successful production of quartz glass, and will be considered in a later paper.

One other conclusion appears to be reasonably certain from our work, namely, that air once enclosed within the body of a charge of quartz glass can not be displaced, either by long-continued heating or by extremely high temperatures.

Our experience did not suggest that we were approaching any necessary limit in the size of the charge which could be handled. A furnace of suitable size, provided with somewhat more power, would undoubtedly produce clear quartz glass in much larger units than we were able to do in our small furnace.

Summing up the conditions for preparation of good quartz glass, we find them to be: An initial temperature of 2,000° or more, without pressure, to produce sufficient quartz vapor to drive out the air from between the grains, followed by pressure (at least 500 pounds), and a reduced temperature (perhaps 1,800°), with time for the quartz to flow compactly together without being attacked by the graphite.

Arthur L. Day,
E. S. Shepherd.

Geophysical Laboratory,
Carnegie Institution,
Washington, D. C.,
April 18, 1906.

METEOROLOGICAL PHENOMENA ON MOUNTAIN SUMMITS.

Much of our knowledge of the upper air has been obtained from observations made on the summits of mountains. With the single notable exception of the Prussian Aeronautical Observatory near Berlin, where, for several years, daily observations at great heights have been obtained with the aid of kites and balloons, we are still dependent upon the mountain observatories for information concerning annual and seasonal changes in the upper air at different heights, and for other data not easily secured except by means of continuous observations made at the same place. The chief error arising from any general application of such observations is caused by the unknown influence of the mountain itself upon the meteorological conditions in its vicinity. The results from observations in the free air do not show the same vertical changes that are observed on mountains, the diurnal periodic change of temperature noticeable on all mountains disappearing at a height of 1,000 meters in the free air.

A few approximate comparisons have already been made, of Ben Nevis (1,343 meters high) in Scotland by Mr. Dines, and of the Brocken (1,100 meters high) in Germany by Dr. Assmann, but in both instances the kite or balloon observations apparently were made at a distance exceeding 90 kilometers from the mountain observatory. Also, Mr. Clayton has compared the temperature on Blue Hill with that of the free air.

The data obtained indicated that the temperature on mountain summits is lower than that of the free air at the same height. No information as to differences of humidity or wind velocity is available, although it appears quite probable that the wind velocity is higher on mountains than in the free air at the same height.

During the last week in August, 1905, I was able to make a comparison of the weather conditions on Mount Washington, N. H., with those of the free air, by means of kites flown in the Ammonoosuc Valley 16 kilometers west of and 1,500 meters lower than the summit of

the mountain. This valley is open toward the west, whence come the prevailing winds, and there are ample open spaces suitable for kite flying.

The meteorological data were obtained from four meteorographs constructed as nearly alike as possible in order to secure uniform action, all having the same scales and each recording the temperature, atmospheric pressure, humidity and velocity of the wind upon a paper-covered cylinder rotated once in twelve hours by a clock. The time-scale was about 25 millimeters an hour and data could be obtained every two minutes. Two of these instruments were employed in recording the conditions on the summit of Mount Washington and at the kite station near Twin Mountain, and two were adapted for use in the kite experiments, being a modified form of the kite meteorograph devised by me for use at the Blue Hill Observatory. All of the instruments were carefully compared with standards.

The meteorograph on the summit of the mountain was exposed in the north window of the office of the newspaper *Among the Clouds,* the editor, Mr. Frank H. Burt, very kindly volunteering to keep it in operation during the experiment. The exposure, so far as temperature, humidity and pressure are concerned, was very good. The anemometer, which recorded electrically, was placed on the roof of the office near the western end and was well exposed to the winds from all directions except those between northeast and southeast, which were obstructed by the other buildings on the summit.

The kites employed were two Eddy kites respectively of 1.67 and 3.20 square meters area, and two 'Blue Hill box' kites, each having a lifting surface of 2.0 square meters. A 'hand' windlass containing 3,400 meters of No. 11 music wire, and provided with accurate devices for indicating the length of line employed and for recording the pull of the kites, was employed in flying the kites. The heights reached were chiefly determined from altitudes obtained by means of a transit, though intermediate heights could be obtained from the record of atmospheric pressure.

It was intended to keep one of the kite meteorographs as nearly as possible at the same height as the summit of Mount Washington, and the other about half as high, and in this way obtain a vertical section of about 1,500 meters; the Twin Mountain station being 427 meters and the summit 1,916 meters above sea level.

The instruments at the summit and kite stations were compared with a standard thermometer three times each day and comparisons of the kite meteorographs were made at the beginning and end of each flight.

After the apparatus was installed there remained but five days for the actual work of observation, and to my surprise, the wind was so exceedingly light throughout the entire period that but two flights could be obtained; during one of which, however, the upper meteorograph was carried to within 60 meters of the height of Mount Washington. An accident to one of the kites prevented more than one observation at this height; but since this appears to be the first time such observations have been made so near a mountain observatory, it may be worth while giving the results in detail.

AUGUST 24, 1905.

Time P. M.	Conditions at Kite.				Conditions on Mt. Washington, 1,489 Meters above Valley.		
	Height above Valley, in Meters.	Temperature, °C.	Relative Humidity, Per Cent.	Wind Velocity, Meters per Second.	Temperature, °C.	Relative Humidity, Per Cent.	Wind Velocity, Meters per Second.
3:15	0	22.2	90	6	7.2	5?	19
3:26	267	19.4	90	6	7.2	5?	19
4:11	456	15.6	85	9	6.1	4?	20
4:17	658	14.4	100	13	6.1	20?	18
4:38	970	11.7	80	14	5.6	25	17
5:02	1,135	11.1	75	12	6.1	28	17
5:31	1,224	11.0	65	12	6.1	30	15
5:41	1,408	10.0	65	13	6.1	30	16
5:43	1,428	9.4	65	13	6.1	33	16

At 5:44 P.M. one of the two supporting kites collapsed and the other, not being sufficient to support the meteorograph and line until they were reeled in, fell into the forest on the north slope of the 'Three Sugar Loaves' (a small mountain 300 meters higher than the kite station) and the flight came to an abrupt end.

Allowing for the difference of level of 61 meters, the observations indicate a decidedly lower temperature and a much higher wind velocity on Mount Washington than are found in the free air. The hygrometers had not been tested below 40 per cent. and the comparison of humidities, while indicating a lower humidity on the mountain, is not considered trustworthy.

Unusually clear, fine weather prevailed throughout the time that could be devoted to the experiments and the summits of the mountains were seldom hidden by clouds. On two successive days (August 25 and 26) the average wind velocity on Mount Washington was less than three meters per second, and on several other days it was almost as low. The conditions for kite flying may not be more difficult near mountains than in other places, but the consequences of accidents to the kites, and the fall of the line and apparatus into the dense forests in these regions, demand that unusual precautions be taken to avoid mishaps. A small gasolene motor for quick manipulation of the line during periods of light wind is almost a necessity.

These experiments have also demonstrated the great value of a simple and compact meteorograph in obtaining data at a place like Mount Washington. The time required for changing the records of the instrument employed was about five minutes, or less, each day, the construction of the recording mechanism of the anemometer being such that this operation could be performed at any time convenient to the observer, or even omitted for a day without loss of records by superimposing. After the meteorograph was installed, on August 20, continuous records of the four elements already referred to were maintained until the close of the season on the summit. It was not practicable to record the direction of the wind on this meteorograph except by means of a device indicating only the eight principal directions; and while such approximate data are useful in studies of climate they are of small value in other meteorological researches.

If circumstances favor, this work will be continued during at least three weeks of the summer of 1906, and, it is hoped, more definite results will be obtained than those described in this preliminary study.

I am indebted to Mr. H. H. Clayton for the use of four Blue Hill box kites; to the staff of *Among the Clouds,* Messrs. Burt, Dunham, Libby and Duff, for their assistance in caring for the meteorograph on Mount Washington, without which these experiments could not easily have been made; to Mr. D. J. Flanders, of the Boston and Maine Railroad, for transportation over the Mount Washington Railway for the purpose of installing the meteorograph on the summit; to the foreman at the Rosebrook Inn, and to Mr. Anderson, of the American Museum of Natural History, for valuable assistance in the kite flights.

S. P. Fergusson.

Hyde Park, Mass.,
February 25, 1906.

QUOTATIONS.

THE CALIFORNIA UNIVERSITIES.

The amended report of the condition in which Leland Stanford, Junior, University is left by the earthquake is most comforting. But if the first statement that its buildings had all been reduced to heaps of dust had proved true the university would not have been, as the headlines had it, 'wiped out.' A university does not exist in its material part. The plant is, in fact, the least part of it. Perhaps it would have been worth the sacrifice of the beautiful Boston-planned architecture of Leland Stanford, Junior, University representing Hispano-Mexican history and the semi-tropical local color of California as vividly as the architecture of Harvard and Yale represent the associations of old New England with Cambridge and Oxford universities, if the impressive object-lesson had been conveyed to our 'splendid materialism,' that the buildings, though they may represent many millions and that in irreproachable good taste, too, do not make and can never make the university. The Leland Stanford Junior University is what it is not by grace of Leland Stanford's money, but by virtue of certain great and fearless minds, with their unwaver-

ing devotion to the highest, with their deep and comprehensive grasp of the relations of the present to the past, the local to the whole world of mankind; with their sense of duty to set the feet of the oncoming generations of Americans in paths laid out in accord with the true laws of growth as far as science can settle what the true course may be and in obedience to the highest and broadest moral and social purpose and responsibility. This direction has been maintained at Leland Stanford, Junior.

Its endowment of thirty millions, its site covering nine thousand acres overlooking San Francisco and the Pacific, thirty miles away, are superb indeed, in all senses of the word. But the animating genius is that of David Starr Jordan, a man with something very much like the physical, mental and spiritual endowment of Phillips Brooks. There would be no question raised to the statement that the building up and development of this university is due mainly to the work and the personal equation of President Jordan, who has been its only president. We had last winter, in one of the Lowell Institute lecture courses, an interesting type of the Pacific coast college professor in Dr. Henry Morse Stevens, of the University of California, with his fascinating review in twelve lectures on the growth of humanitarianism in the world since Francis of Assisi and its developments in charities and corrections. It is still fresh in mind—the powerful impression produced here in Boston by this new authority for us—the scholarship and above all the social purpose revealed in a remarkable series of papers demonstrating from history, in a spirit of broad and dauntless optimism, that the state is constantly taking upon itself to see that the world does really grow better through feeling a closer responsibility for its defectives, and that patriotism must be expanded beyond a narrow nationalism in the scientific interpretation of history. With such enlightenment flowing forth daily upon the four or five thousand students of the great university patronized by Mrs. Phœbe A. Hearst, and similar influences shed from the greatmindedness of President Jordan upon about half as many in that endowed by Mrs. Stanford, a large proportion of all of whom are young women, it is to be gathered that the 'Coast' is taking on an intellectual and social culture deeper than anything that can be toppled into ruin by mere destruction of buildings.—*The Boston Transcript.*

THE CONGRESS OF THE UNITED STATES.

April 6, 1906.—A bill passed the Senate to incorporate the Archeological Institute of America.

A bill passed the Senate to appropriate twenty-five thousand dollars for the establishment of a fish cultural station in the state of Kansas.

April 9, 1906.—Senate bill, 3,245, creating the Mesa Verde National Park, after amendment, passed the Senate.

April 11, 1906.—Senate bill, 4,487, granting to the state of Oregon certain lands to be used by it for the purpose of maintaining and operating there a fish hatchery passed the Senate.

April 13, 1906.—The bill to incorporate the Archeological Institute of America, which passed the Senate, has been referred to the Committee on Foreign Affairs, in the House of Representatives.

April 17, 1906.—A bill to prohibit aliens from fishing in the waters of Alaska passed the House, with amendments.

THE AMERICAN PHILOSOPHICAL SOCIETY.

THE American Philosophical Society held an extremely interesting meeting last week in commemoration of the Franklin Bicentenary. The program has been printed in SCIENCE, and we hope to print later an official account of the proceedings. New members were elected as follows: The Hon. J. H. Choate, LL.D., Dr. H. H. Donaldson, professor of neurology in the Wistar Institute of the University of Pennsylvania; Russell Duane, lecturer in the Law School of the University of Pennsylvania and a lineal descendant of Benjamin Franklin; Dr. D. L. Edsall, assistant professor of

medicine in the University of Pennsylvania; Dr. C. S. Hastings, professor of physics in the Sheffield Scientific School of Yale University; Dr. W. F. Hillebrand, chemist in the U. S. Geological Survey; Charles Rockwell Lanman, LL.D., professor of Sanskrit and comparative philology in Harvard University; Dr. F. P. Mall, LL.D., professor of anatomy in the Johns Hopkins University; the Hon. Elihu Root, LL.D., secretary of state; Dr. E. F. Nichols, professor of experimental physics in Columbia University; T. D. Seymour, LL.D., professor of Greek in Yale University; Dr. E. B. Titchener, professor of psychology in Cornell University; O. H. Tittmann, superintendent of the U. S. Coast and Geodetic Survey, and Dr. A. G. Webster, professor of physics in Clark University.

The University of Pennsylvania conferred the degree of doctor of science on William P. Hemszey, the engineer, and on James Gayley, the analytic chemist and trustee of Lafayette College. The degree of doctor of laws was conferred on King Edward VII.; Guglielmo Marconi, inventor of wireless telegraphy; Andrew Carnegie; George H. Darwin, professor of astronomy in Cambridge University; Edgar F. Smith, professor of chemistry in the University of Pennsylvania and president of the American Philosophical Society; Hampton L. Carson, attorney general of Pennsylvania; J. W. Mallet, professor of chemistry in the University of Virginia; Wm. B. Scott, professor of geology and paleontology at Princeton University; E. C. Pickering, professor of astronomy and director of the Harvard College Observatory; Hugo de Vries, professor of plant anatomy and physiology in the University of Amsterdam; A. A. Michelson, professor of physics in the University of Chicago; Ernest Rutherford, professor of physics in McGill University; E. L. Nichols, professor of physics in Cornell University; W. K. Brooks, professor of zoology in the Johns Hopkins University; W. P. Patterson, professor of divinity in Edinburgh University; Professor H. A. Lorentz, professor of mathematical physics in the University of Leiden; Alois Brandl, professor of philology in the University of Berlin; Samuel Dickson, chancellor of the Law Association of Philadelphia.

SCIENTIFIC NOTES AND NEWS.

The appalling disaster on the Pacific Coast has completely spared the University of California and the Lick Observatory. The buildings of Leland Stanford Junior University have suffered severely, the loss being estimated at $4,000,000. The building of the California Academy of Sciences and its valuable collections were destroyed.

A dinner in honor of Professor H. A. Lorentz, of the University of Leiden, was given by the Philosophical Society of Washington, on the evening of April 21.

The University of St. Andrews has conferred its doctorate of laws on Dr. A. C. L. G. Günther, formerly keeper of the Zoological Department of the British Museum, and on Dr. A. H. Young, professor of anatomy at Manchester.

The United States ambassador to Great Britain, Mr. Whitelaw Reid, presented the gold medal of the American Geographical Society to Captain R. N. Scott, commander of the National Antarctic Expedition, on April 9.

Dr. Hobart Amory Hare and Dr. Francis Xavier Dercum entertained recently as guests of honor at dinner at the Art Club, Philadelphia, Drs. E. Anthony Spitzka and George McClellan, recently appointed professors of anatomy in Jefferson Medical College.

Mr. H. H. Clayton, meteorologist of the Blue Hill Meteorological Observatory, has accepted the position of professor in the U. S. Weather Bureau, and will assume his duties in Washington on about July 1.

Mr. A. F. Crider, of the United States Geological Survey, has been appointed state geologist of Mississippi and professor of geology in the university of the same state. The line of work first undertaken by the state survey will be an investigation of the cement resources, the clays and the lignites.

Dr. Alfred W. G. Wilson has resigned his appointment as demonstrator in geology at

McGill University to go into private practise as a consulting geologist in engineering and mining work. His present address is 197 Park Avenue, Montreal.

Nature states that at a meeting of the council of the Royal College of Surgeons of England, held on April 5, the Walker prize of £100, founded by the late Mr. C. C. Walker to encourage investigation into the pathology and therapeutics of cancer, was awarded to Professor C. O. Jensen, of Copenhagen. The committee appointed to advise the council in reference to the award of the prize was influenced, not merely by the actual work which Professor Jensen has done in investigating the nature of cancer and the effect of treatment upon it, but also by the extent to which he has opened up a field of research to those engaged in the study of cancer on certain lines, enabling them to carry out their investigations over longer periods of time and under better and more determined conditions than have up to the present time been possible. The Jacksonian prize for 1905 was awarded to Mr. R. C. Elmslie for his essay on 'The Pathology and Treatment of Deformities of the Long Bones due to Disease occurring during and after Adolescence.' The prize-subject for the year 1907 will be 'The Operative Surgery of the Heart and Lungs, including the Pericardium and the Pleura.' The subject selected for essays to be submitted in competition for the Cartwright prize for the period 1906–1910 was 'Prevention of Dental Caries.' The honorary medal of the college was awarded to Lieut. Colonel Sir Richard Havelock Charles, I.M.S., in appreciative recognition of his gift of anthropological specimens—an addition to the museum of special value and importance, not only on account of the number and variety of the specimens presented, but also because of the authentic particulars attached to them.

PRESIDENT JORDAN, of Stanford University, gave recently the convocation address at the University of Wisconsin, the subject being 'The Call of the Twentieth Century.'

MR. A. LAWRENCE ROTCH, director of the Blue Hill Meteorological Observatory, gave an illustrated lecture before the Middletown Scientific Association, on April 10, entitled 'Recent Investigations at Great Heights above the American Continent and the Atlantic Ocean.'

PROFESSOR LOUIS KAHLENBERG, of the University of Wisconsin, delivered, between April 9 and 13, a series of five lectures in physical chemistry before the faculty and students of the College of Science at the University of Illinois. The subjects of the lectures were: 'The relation between chemical action and electrical conductivity,' 'Osmosis and dialysis,' 'The rôle of silicates in nature,' 'A study of the optical rotatory power of substances' and 'The nature of solutions.' Throughout the series the lecturer argued for the recognition of solutions as chemical compounds according to variable proportions. On the evening of April 12, Professor Kahlenberg was given an informal reception by the faculty of the department of chemistry.

A SPECIAL number of the *University of Chicago Record* has been issued as a memorial to President Harper. The issue, which consists of ninety pages, contains appreciations, and also the chief addresses delivered at the various memorial services held at other American universities. Four portraits are given.

M. PIERRE CURIE, professor of physics at the Sorbonne, Paris, eminent with Mme. Curie for the discovery of radium, was run over and killed by a wagon in Paris, on April 19. M. Curie was born on May 15, 1859.

WE regret also to record the deaths of Dr. Tullio Brugnatelli, professor of chemistry at the University of Pavia and of the Swiss ornithologist, Victor Fatio.

THE next meeting of the Astronomical and Astrophysical Society of America will be held at New York, in affiliation with the American Association for the Advancement of Science, during convocation week, 1906–7.

THE working library of Professor Meissner on internal medicine, and a botanical library of three hundred or more volumes, consisting mainly of old and classical works on herbs,

have been given to the Newberry Library, Chicago, by Dr. Nicholas Senn.

The American Academy of Arts and Sciences is the custodian of two funds, one known as the Rumford fund, the other, the Warren fund. The Warren fund consists of ten thousand dollars left to the academy by the late C. M. Warren, the interest of which, about four hundred dollars a year, is, according to the will of the donor, used for the encouragement of chemical research. A committee appointed by the academy, known as the C. M. Warren Committee, receives and considers applications for grants from this fund and reports its action to the academy at the annual meeting in May, for approval. Applications should be made to Professor Leonard P. Kinnicutt, Worcester Polytechnic Institute, Worcester, Mass., stating exactly the scope of the research for which aid is asked, and also a statement as to the way the money is to be used in carrying out the research. The recipients of aid from this fund are expected to send to the chairman of the committee, Professor Kinnicutt, at the end of each year, a report of the work accomplished, and to mention in any publication of the research that aid had been given for carrying on the work from the C. M. Warren fund of the American Academy of Arts and Sciences.

Consul Griffith, of Liverpool, transmits a report on the establishment of an institute of tropical research, the objects of which are the collecting and tabulating of all kinds of information regarding tropical countries, their products, natural resources, industries and economic conditions, which can be of service either to commerce or science. The consul says that no provision has heretofore been made in Europe for dealing in a systematic manner with the scientific study of the tropics and of their economic aspects as a whole. The Liverpool institute represents the first effort to systematically collect and collate accurate knowledge concerning the tropics and place the result of this expert research work in an accessible form.

We learn from *The British Medical Journal* that the governor general of the Soudan has appointed a commission to investigate the possibility of the extension of 'sleeping sickness' into Soudan territory. The commission is to consist of Lieutenant-Colonel G. D. Hunter, D.S.O., P.M.O., Egyptian Army; Dr. Andrew Balfour, director of the Wellcome Research Laboratories, Khartoum; a British medical officer of the Egyptian Army, or a medical inspector of the Soudan Medical Department, or such members as may be hereafter appointed. The points to be investigated are: (1) To ascertain the distribution of various species of tsetse flies or other biting flies in the Soudan; (2) to ascertain if the disease at present exists in Soudan territory—if so, to determine the exact areas, and to what extent the distribution of the disease coincides with the presence of the tsetse or other flies in these areas; (3) a systematic investigation of the blood of a population in an infected district; (4) a thorough and complete research into the character of the disease, especially as regards its origin and spread.

The following are the lecture arrangements at the Royal Institution after Easter: Professor G. Baldwin Brown, two lectures on Greek classical dress in life and in art; Professor William Stirling, three lectures on glands and their products; Dr. P. Chalmers Mitchell, two lectures on the digestive tract in birds and mammals; the Rev. J. P. Mahaffy, two lectures on (1) the expansion of old Greek literature by recent discoveries, (2) the influence of ptolemaic Egypt on Græco-Roman civilization; Professor William J. Sollas, three lectures on man and the glacial period; Professor Charles Waldstein, three lectures on English furniture in the eighteenth century; Sir James Dewar, two lectures on the old and the new chemistry; and Professor W. Macneile Dixon, two lectures on (1) the origins of poetry, (2) inspiration in poetry. The Friday evening meetings will be resumed on April 27, when Professor John W. Gregory will deliver a discourse on ore deposits and their distribution in depth. Succeeding discourses will probably be given by the Hon.

Charles A. Parsons, Professor J. H. Poynting, Professor Arthur Schuster, Mr. Leonard Hill, Professor H. Moissan, and Sir James Dewar.

A RECENT Friday evening lecture at the Royal Institution was given by Professor P. Zeeman, of Amsterdam University, on 'Recent Progress in Magneto-Optics.' According to the London *Times,* Professor Zeeman gave a general review of the experimental researches on the relation betwen magnetism and light which had occupied him during the last few years. His observation, made in 1896, of a slight widening of the spectral lines of sodium under the influence of a magnetic field was, he said, the origin of his work, which he carried on in the light of the theory of electromagnetic and optical phenomena developed by H. A. Lorentz. In accordance with this theory he found that in a strong magnetic field certain spectral lines were divided into three components, when the magnetic force was at right angles to the direction of propagation of the light, and further, that the middle one of these components was plane-polarized in a direction different from that of the two outer ones. When the magnetic force was parallel to the direction of the propagation of the light, the lines split up into two components, each circularly polarized but in opposite directions. From these phenomena it could be inferred that in a luminous gas all vibrations arose from the negative electrons, and the value deduced for the ratio of the charge to the mass of the electron was of the same order as that obtained from the study of the cathode rays. Professor Zeeman next considered the rotation of the plane of polarization close to an absorption band, and then the double refraction and resolution of the absorption lines. Finally, he discussed the behavior of different spectral lines in the magnetic field. In many metallic spectra a number of the lines occurred which were closely related and formed so-called series. It was found that all lines of the same series were split up in the same manner, *e. g.,* all were resolved into triplets, or sextets, or nonets; moreover, not only was the general type of subdivision the same, but even the amount of separation, measured in oscillation frequency. A second law was that the corresponding series of different elements also showed the same type of resolution and the same amount of separation. The conclusion seemed to be that all the lines of a series were emitted by one oscillating system, and that, therefore, there were as many series in the spectrum of a substance as there were oscillating systems in its atom; and that the oscillating mechanism was the same in different elements. He thought there could be no doubt that spectrum analysis and especially the magnetization of the spectral lines would give a clue to the inner structure of the atom.

The Scottish Geographical Magazine states that for some time past preparations have been made for a French Colonial Congress, to be held at Marseilles in September next, under the presidency of M. J. Charles Roux, the well-known writer on colonial subjects. From the intimate connection which exists between marine and colonial affairs, it has been decided to extend the scope of the congress by the addition of an exhibition, intended to illustrate all matters connected with the scientific study of the sea and its fisheries. Its organization was entrusted to M. Charles Bénard, president of the Oceanographical Society of the Golfe de Gascogne, who has done more than any one else to further the study of oceanography in France within recent years. In recognition of the fact that the sea knows no political boundaries, it was wisely decided to give the exhibition an international character, and the cooperation of the leading oceanographers of all nations was invited. A British committee was formed under the presidency of Sir John Murray, including representatives of the principal organizations connected with the study of oceanography and marine biology in this country, and under its auspices a representative British exhibit has been got together. The society will be represented at Marseilles by Captain Wilson Barker, and among other exhibits, has sent the model of the Antarctic exploring ship, *Discovery,* a special feature of the exhibition being the illustration of the great

scientific exploring expeditions. It will include—besides examples of the best scientific instruments and appliances, charts, photographs, etc.—a number of sections devoted to the industrial side of the subject: the equipment of fishing-vessels, appliances for the capture and preservation of fish, life-saving apparatus, and many other classes of objects. A congress of geographical societies and of the 'Alliance française' (an association for the extension of the French language in the colonies and abroad) will also be arranged, the geographical section being under the presidency of M. le Myre de Vilers, president of the Paris Geographical Society. Its proceedings will be devoted towards furthering the spread and advancement of geographical science.

UNIVERSITY AND EDUCATIONAL NEWS.

The University of California has received a gift of $100,000 from the widow of the late Judge John H. Boalt.

Mr. Andrew Carnegie has offered $40,000 to Denison University for a new library building on condition that a like sum be secured elsewhere for the endowment of the library. It is expected that the condition will be met and construction begun soon.

Through the generosity of Mr. Robert S. Brookings and Mr. Adolphus Busch, the Medical Department of Washington University (St. Louis) has received a gift of $50,000.

The Studies and Examination Syndicate of Cambridge University has presented a report recommending that students of mathematics and science may be exempted from the entrance examination in Greek. Students of science would receive the degree of bachelor of arts and science and other students the degree of bachelor of arts and letters.

Professor Andrew Cunningham McLaughlin, professor of American history in the University of Michigan, has been appointed professor and head of the department of history in the University of Chicago. The headship of the department has been vacant since last summer, when Professor Jameson resigned to become director of the bureau of historical research in the Carnegie Institution, the position previously held by Professor McLaughlin.

At a recent meeting of the regents of the University of Wisconsin a number of appointments were made and provision for additional professorships. Professor W. D. Pence, now head of the department of civil engineering at Purdue University, was elected to the chair of civil engineering, to fill the vacancy caused by the resignation of Professor W. D. Taylor, who has become chief engineer of the Chicago and Alton Railway. Dr. Edward B. Van Vleck, now professor of mathematics at Wesleyan University, was appointed to the professorship of mathematics left vacant by the resignation of Professor C. A. Van Velzer. Upon recommendation of the regent committee on the college of agriculture, George N. Knapp, assistant professor of farm engineering, was removed. A number of other appointments and several promotions were made at this meeting of the regents. Dr. Thomas S. Adams was promoted from assistant professor to associate professor of political economy; Emmett D. Angell from instructor to assistant professor of physical culture; Eliot Blackwelder from instructor to assistant professor of geology; Boyd H. Bode from instructor to assistant professor of philosophy; Charles W. Stoddart from instructor to assistant professor of soils. The new instructors appointed were: E. R. Jones, soils; W. G. Marquette, botany; T. Sidney Elston, physics; George N. Northrop, English; Herman T. Owen, music; L. J. Pactow, history. The assistantships filled included: Julian P. Blackman, physiology; A. R. Harris, official tester in agricultural chemistry; J. G. Brandt, Latin; J. L. Conger, American history; D. R. Lee, Latin; James Milward, horticulture; and Charles W. Hill, chemistry.

Professor E. H. Starling has been appointed to the Jodrell chair of physiology in the University of London.

Mr. G. C. Gough, B.Sc., has been appointed to the chair of natural history at the Royal Agricultural College, Cirencester, vacant by the resignation of Professor West.